

THE PSYCHOLOGICAL REVIEW

CONTINUOUS VERSUS NON-CONTINUOUS INTERPRETATIONS OF DISCRIMINATION LEARNING

BY KENNETH W. SPENCE

State University of Iowa

I

Some few years ago the writer (14) presented a theoretical interpretation of discrimination learning based upon stimulus-response concepts and association principles which he contrasted with a purely descriptive picture of the behavioral phenomena of the pre-solution period of such learning offered by Krechevsky (5). Considerable care was taken in the article to point out that Krechevsky's discussion of the problem was entirely a descriptive affair and as such had a different purpose than the present writer's interpretation. That Krechevsky concurred with this difference in approach is shown by the following quotation from a subsequent note of his:

In the first place I should like to point out that as far as Spence's original contribution is concerned there need not be any point of conflict between what he has to offer and anything I may have said in my previous papers. This does not mean that I necessarily accept Spence's proposed theoretical 'explanation' of my data, but simply that we are really talking about two different things. I had contented myself with an attempt at a description of the learning process on a given behavioral level (6, p. 97).

After pointing out this purely descriptive characteristic of Krechevsky's interpretation, the present writer went on to propose and elaborate a second, alternative theory as to the nature of pre-solution behavior which, it was believed, was in line with the original statement by Lashley that "the practice preceding and the errors following are irrelevant to the actual

formation of the association" (8, p. 135). This theory¹ was that the animal sets out to solve the problem confronting it by trying out, systematically, one of its repertoire of hypotheses. If this fails of solution, the animal is led to initiate and try out another and another until the correct one is hit upon (14, p. 444). In other words, this theory assumed that the animal *selects* and responds, in turn, to certain aspects of the experimental situation as offering possibilities of providing a solution and that it does not react (pay attention) to the relevant cue aspect until just at or just preceding the time of solution. Although incomplete in many respects, this conception leads to the implication that if the values of the cue stimuli are reversed, *i.e.*, the positive stimulus made negative and vice versa, before the animal begins to show any learning, it should not necessarily make for any slower learning of the reversed problem.

The present writer proposed this extension of Lashley's suggestion as one which disagreed with the implications of his own association type of theory and then proceeded to examine the experimental evidence bearing on the issue. Since that time several articles have been published by Krechevsky (7) and Haire (2, 3) which have dealt in one way or another with this problem. While in some instances these have provided valuable experimental contributions, they have, unfortunately, been marred by a tendency to misinterpretation of the opposing viewpoint, with the result that a very misleading picture has developed as to the status of the issue. It is the purpose of the present paper to attempt to clarify these confusions and also to examine the experimental data, old and new, which bear upon the problem.

¹ This theory which the writer dubbed the insight hypothesis is referred to here as the Lashley theory because it is based upon Lashley's original statement quoted above. Krechevsky (6, p. 98-100) mistakenly believed that the writer ascribed the theory to him and somewhat indignantly denied such responsibility. This was a misinterpretation on his part. The writer's statement was: "But whatever Krechevsky's view of the matter may be, one can, nevertheless, advance the hypothesis that . . ." (14, p. 444). The subject of this sentence was 'one,' not 'Krechevsky.' Furthermore, the theory or hypothesis was referred to as the insight hypothesis, not as the Krechevsky hypothesis. It is the writer's belief that the theory as outlined is merely a simple, logical extension of Lashley's brief statement.

II

We may begin with the results of the original McCulloch and Pratt experiment on weight discrimination with white rats (13), which the writer has already cited as supporting his association theory of discrimination learning and as being in conflict with the Lashley theory (14). [These investigators found that when the significance of the cue stimulus was reversed before the subjects were responding more than 50 per cent to the positive cue stimulus, the subsequent learning of the converse problem required a longer time than a control group, continued on the original problem. A second experimental group which was trained originally until it was just beginning to discriminate, subsequently required even more trials than the first group to learn the converse problem. McCulloch and Pratt interpreted this to mean that instead of being docile and insightful at the moment of discovery of the relevant stimulus cues, the rats, before solving the problem, continued on the now incorrect cue and even went back to responding systematically to stimuli (position) that had previously been given up.

But Krechevsky (6), writing later, raised an important point, which, if found to be true for these data, would render them indeterminate so far as the Lashley theory is concerned. His point was that the animals might have been responding to more than one hypothesis at a time, one of which was the correct stimulus. He wrote:

An animal *may* (and in many cases probably does) select only one cue out of the stimulus field, and in such cases a change in the value of the cue stimulus may be without effect on the learning speed; but he may also (and that I have demonstrated) before dropping one hypothesis entirely, or to any great extent, select another cue and continue to divide his responses between the two. In this latter case, obviously, a reversal of the meaning of the second cue (if it happened to be the correct one) would interfere with the learning speed of the animal. Nothing that I have said rules out either of the two possibilities—actually from the data I have presented, *both* of those possibilities would be expected (6, p. 103).

If the animals had been responding to two stimuli or hypotheses, position and weight, at the same time, then reversal of the significance of the stimuli, it is true, should be expected to interfere with the converse problem. But to prove his point it was necessary for Krechevsky to show that the animals actually were responding to a weight cue. This he made no attempt to do despite the fact that there was a perfectly simple method of checking for it. Since McCulloch and Pratt reported, that with the exception of one case, their subjects did not respond more than 50 per cent of the time to the positive weight cue before the reversal, it is obvious that they could not have been responding to either one of the weight stimuli in combination with any other stimulus.

More recently, however, Krechevsky (7) has presented experimental evidence in visual discrimination which is contrary, in part, to McCulloch and Pratt's data and thus in support of the Lashley theory. Apparently encouraged by this turn of affairs Krechevsky was himself led to propose a Lashley type theory similar in its essential details to that previously suggested by the present writer. According to Krechevsky's formulation of this theory, which he called the non-continuity hypothesis,² each time the animal, while 'paying attention' to a particular set of discriminanda, makes a correct or incorrect response he learns something about the particular stimulus to which he was reacting or paying attention. But he does not learn anything during such trials about the to-be-finally-learned stimuli. He eventually gives up responding to the first set of stimuli and responds to another and another until he hits upon the relevant set. A corollary of the theory, Krechevsky further indicated, is that "if the significance of the stimuli are reversed before the animal begins to 'pay attention to' them (*i.e.*, during the pre-solution period),

² To avoid confusion the terms continuity and non-continuity suggested by Krechevsky will be used hereafter to designate the rival theories. Thus, what has been referred to as the Lashley theory, whether in the form elaborated by Krechevsky (7) or by the present writer (14) is a non-continuity theory. The writer's association theory of discrimination learning (14) is one of several versions of the continuity theory. Other theories included in this class by Krechevsky are those of Gulliksen and Wolfe (1) and Wiley (19).

it should not necessarily make for any slower learning of the reversed problem" (7, p. 112).

In contrast to his version of the Lashley theory, Krechevsky presented what he called the continuity hypothesis, citing as one example the association theory of discrimination learning proposed by the writer (14). His statement of the continuity theory was as follows:

Simply restated the former assumption (which we shall from now on refer to as the 'continuity' assumption) says that once an animal is immersed in a given problem situation (more specifically, discrimination-box) *each time* the animal makes a choice an effect is recorded on his nervous system. This effect is specific and constant. Every time, from the very first trial on, the animal makes a 'correct' response the strength of the tendency to respond positively *to the specific stimulus the experimenter has in mind*, is strengthened; each time, also from the very beginning, he makes an 'incorrect' response, the strength of the tendency to respond positively to the specific stimulus the experimenter has in mind is weakened, and further, these increments (or decrements) are constant throughout the learning process. Nothing is said about the 'attention' of the animal, nothing about the 'awareness' of the animal with respect to the important stimuli (7, p. 111).

Insofar as the above statements are supposed to apply to the present writer's version of the continuity type of theory, there are three major deviations. [First, the effects of each trial (correct or incorrect) have not been assumed to be constant.] It is difficult to understand how Krechevsky could have made such a mistake, for the writer very explicitly postulated that the increments of reinforcements and decrements of non-reinforcement varied with the strength of the associative connection, and a table showing these relationships was published (14). Apparently Krechevsky missed the significance of the writer's theoretical account of the pre-solution phenomena in discrimination learning, for it is only on the basis of these assumed *variable effects of reinforcement* that they can be explained.

Secondly, the writer did not assume that, from the very first trial on, the strength of the tendency to respond to the specific stimulus the experimenter has in mind is necessarily strengthened. The following quotation reveals the inapplicability of this statement. "The mere presence of the cue stimulus somewhere in the experimental situation does not guarantee its impingement on the animal's sensorium at or near the critical moment of response. This was, undoubtedly, an important, if not the chief reason for the greater difficulty that was experienced in setting up discrimination habits in the older forms of indirect apparatus" (14, p. 438).

The third discrepancy between the writer's particular version and Krechevsky's outline of the continuity hypothesis is his complaint that nothing is said about the attention of the animal, nothing about the awareness of the animal with respect to the important stimuli. This statement of Krechevsky's, when written, was accurate, but was not at the time of its publication. In September, 1937, almost six months prior to the publication of Krechevsky's paper, the writer attempted to deal with this question in a brief footnote (16, Footnote 3). The problem is an important one, particularly for the experiment being considered here, which is crucial for the continuity type of theory only if one can be sure the relevant stimuli are received on the animals' sensorium each trial from the very first. A more extended discussion of it will be given here in order to clarify the writer's view.

Insofar as the discrimination situation is such that the animal's sense organ is not forced to receive the relevant aspect of the stimulus from the very first trial, associations would not be formed between the response and this particular stimulus aspect. Obviously, if the stimulus does not occur for the animal, it cannot acquire any associative connections. The design of discrimination apparatus has been directed towards the forcing of the reception of the relevant stimulus aspects from the beginning of training. In the case of weight discrimination and, in the field of vision, at least, in brightness discrimination, the modern types of apparatus and procedures have been more or less successful in this respect, for it is prac-

tically impossible for the animal to make a response in such situations and not receive the relevant stimulation. In the case of visual discrimination of forms or figures, on the other hand, this condition does not necessarily hold. In these instances the animal is required to learn, in addition to the final selective approaching response, the appropriate (perceptual) response which leads to the reception of the relevant stimulus aspects. That is to say, the animal must learn to orient and fixate its head and eyes so as to receive the critical stimuli. These reactions are learned for the very same reason that the final approaching response is learned, *i.e.*, because they are followed within a short temporal interval by the final goal response. Responses providing other sensory receptions are not similarly reinforced in a systematic fashion and hence tend to disappear.

Anyone who has ever observed white rats performing in the jumping style of apparatus or any type involving locomotion (walking) can well appreciate what little likelihood there is that the animal will from the first respond to (look at) a small figure or form in a vertical plane in front of it on each and every trial. The white rat, in particular, runs with its head lowered to the floor and, even in the Lashley jumping apparatus, jumps to, and hence presumably fixates, the lowest portion of the stimulus cards.

Another matter of importance is the brightness relation of the figure and ground. Because animals (white rats, at least) apparently tend to orient visually more readily towards the lighter region of the stimulus complex (10) they are more likely to 'see' or will come to respond more quickly to the form or figural aspect of a stimulus pattern if the figure is light with the background dark than vice versa. All these factors must be considered when dealing with the particular type of critical experiment being considered here. Obviously, conditions can easily be arranged so that the animal will not look toward the relevant stimulus aspect at first, with the result that it will have no effect.

The problem of perception in the discrimination situation is not a simple matter, as Lashley (9) has recently so well

demonstrated, and the above treatment does not make any claim to being exhaustive. The motor responses involved in the orientation of the animal's head and eyes probably play, through their proprioceptive consequences, a much more important part in determining behavior than has been realized. Experimentation should be specifically directed to this important problem. Furthermore, the manner in which complex patterns are reacted to is not dealt with at all. Indeed the whole problem of patterning is omitted from discussion because our present interest is centered on the learning process *in situations where the perceptual response is as simple as possible.*

III

Turning now to Krechevsky's experimental results (7), it may be noted first that the experiment was essentially the same as that of McCulloch and Pratt, except that it involved visual instead of weight discrimination. Briefly, it consisted of training 3 groups of rats with the Lashley jumping technique. A control group (I) was trained throughout to a stimulus card (*A*) containing horizontal rows of black squares on a white background, the negative stimulus (*B*) consisting of vertical rows of black squares. Two experimental groups were trained positively to stimulus *B*, Group II for 20 trials and Group III for 40 trials and were then reversed to stimulus *A* and trained to learning on it. The critical experimental question was whether the animals which were originally trained on stimulus *B* would show retardation in their learning of the converse problem.

Krechevsky presented the data in two ways. He first compared the learning data of the reversed groups (II and III) after reversal with the total learning record of the control group (I), and secondly, he compared the data of the reversal groups with the learning of the control group after 20 and 40 trials, respectively. The first method of comparison is not valid because the experimental groups have had the benefit of 20 (or 40) trials of training by a technique which leads to rapid elimination of position preferences. This technique is

the one which was employed by Lashley and consists in placing the animal, when it responded incorrectly, back on the jumping stand with the same orientation it had before the jump and with the stimuli in the same position until the correct choice was made.³

Its most important effect is to break down or eliminate any position preference or, in the terminology of our theory, it leads readily to equalization of the excitatory tendencies of the two position stimuli (left and right positions), for the continued incorrect response to this stronger position stimulus leads to the weakening of its excitatory strength. That is to say, any animals with position preferences tend to have them equalized by this procedure. But the rate of learning of a discrimination problem is in part a function of the presence and extent of differences in the excitatory strength of the position stimuli. A much smaller difference between the positive and negative cue aspects is required, and hence fewer reinforcements (trials), when the position stimuli are equal in strength than if they are very different. To compare the learning of the experimental groups, which have had their position preferences at least partially eliminated, with the control group which has not, from the beginning of its training, does not take into consideration this factor. The more usual experimental procedure of not permitting such repetitive responses would not tend so readily to result in such elimination of position preferences and hence comparison of such data would not be so seriously affected. But even in such cases the better procedure would probably be to have neither stimulus cue consistently positive during the first 20 (or 40) trial period of the control group. Comparison could then be made from this point on, the experimental group being reversed from stimulus *B* to stimulus *A* and the control group having stimulus *A* made consistently positive.

Confining oneself, then, to these latter data we find that Group II did not require any more trials or errors (in fact, required fewer) than the control group (I) to learn the reverse problem, a result which is in agreement with the non-conti-

³ Krechevsky limited the number of such wrong responses to 5 on the first day.

nuity (Lashley) theory and appears, on the surface, to contradict the writer's continuity theory (as well as all of the other variations of the continuity theory). On the other hand, Group III did require more trials than the control group to learn the converse problem, a result which would appear to be opposed to the non-continuity theory and in support of the continuity theory.

At this point the 'explanations' of the inconsistencies of fact with theory make their appearance. Krechevsky thinks the results for Group II are quite all right; they indicate to him that the animals learned nothing in the two days with respect to the specific cue stimuli. On the other hand, the results of Group III were not, he claims, in disagreement with his theory because, as he states, the results to be expected here on the 'non-continuity' assumption were indeterminate. Everything depended upon whether or not four days (40 trials) were or were not too long for the pre-solution period. Here we encounter one of the most important points in the whole issue. Indeed, whether there is an issue or not depends upon what is meant by such a term as 'pre-solution period,' or the expression "before the animals begin to react to or pay attention to the significant set of stimuli." In his earlier articles, Krechevsky seemed unequivocally to mean by pre-solution period, the period of trials before the animal began to respond *systematically* to the correct stimulus aspect. Thus, responding with an hypothesis or 'paying attention to' a particular stimulus aspect was indicated by the fact that the response to it occurred systematically, *i.e.*, beyond chance expectancy. The pre-solution period marked that period before the animal responded systematically to the relevant stimulus. This was Krechevsky's original concept of hypothesis behavior, being based upon his attempt to find a method of operationally identifying Lashley's 'attempts at solution' on the part of the subject. It was in this sense that the writer originally proposed that the McCulloch-Pratt experiment provided a critical test of the non-continuity (Lashley) theory and the continuity theory proposed by himself. The whole issue was whether or not the presence of the cue stimulus had

any effect before the animal began to respond *systematically* to it. Lashley's suggestion implied that it did not; the writer's theory, that it did.⁴

The fact that Group III learned more slowly following reversal than the control group is definitely opposed to the non-continuity theory in the above sense of the word pre-resolution period, *unless* it can be shown that the animals had already begun to respond systematically to the cue stimuli during the 40 trials. Krechevsky presents no evidence to show that they were doing so. [As McCulloch (11) has pointed out that the control group showed no evidence of responding even above chance (50 per cent) to the positive stimulus in trials 21-40, let alone responding systematically to it, it is not unlikely that Group III were not responding more than chance to either of the cue stimuli. This result then, is, definitely opposed to a non-continuity theory involving the systematic conception of hypothesis.

Returning to the consideration of the results of Group II from the point of view of the writer's continuity theory, the alibi is, of course, that the relevant cue aspects simply were not received at first by the subjects. They did not 'see' the rows of black squares because they were fixating other aspects of the stimulus complex. And in support of this argument attention is directed first to the fact, pointed out above, that the Lashley technique predisposes the animal to fixate and jump to the very bottom of the card, and secondly, that the stimulus patterns involved black figures on white ground. It is the writer's belief that if white squares on a black ground had been used the results for the experiment might have been quite different. However, repetition of the experiment would best be done with white and black stimuli, for in such a situation, with proper precautions, it is practically impossible for

⁴ More recently Haire has suggested abandonment of this strict operational definition of an hypothesis on the truly surprising grounds that "too strict operationism clouds the real issue" (3). In effect, with this restriction removed, Haire is free to posit single or multiple non-operationally defined hypotheses to suit the occasion. Such a theory, whatever its implications, has nothing to do with the type of Lashley theory proposed by Krechevsky and the writer. It is entirely irrelevant to the present issue and, as McCulloch (12) has pointed out, altogether invulnerable to disproof.

the animal to fail to receive the significant stimulation. Clearly, this experiment has its difficulties as a means of testing these two opposing theoretical views.

IV

We turn now to the consideration of an experiment which the writer carried out with chimpanzee subjects (15). The experiment was not at the time of its inception regarded as a critical one for a decision between the association or continuity type of theory and the non-continuity theory, but was designed primarily to test certain additional implications of the writer's own particular theory of discrimination learning. In his paper Krechevsky (7) considers this experiment briefly, and cites various reasons for his belief that it does not have any data relevant to the present problem. In the opinion of the writer, however, Krechevsky has overlooked several points at which this experiment presents data of considerable significance for the present issue.

Briefly, and in part, the experiment was as follows: The subjects were required to learn a series of discrimination problems involving four different stimulus forms, which may be designated *A*, *B*, *C*, and *D* respectively. Each subject was first taught two preliminary discriminations: *A* (+) versus *B* (-) and, after the completion of this, *C* (+) versus *D* (-). Following the learning of these two preliminary problems, the animals were presented with five tests consisting of five new learning problems in which the same stimuli were used in different combinations. Only the first test need be considered for the moment. It involved the positive stimuli, *A* and *C* of the two preliminary problems, half the subjects having *A* positive and half *C* positive.

Commenting on this experiment, Krechevsky remarks that the learning of the preliminary problems meant that the animals had been trained to 'pay attention to' or 'react to' the relevant discriminanda, and that transferring the animals after this to another set of problems involving these same stimuli cannot give us any data on what had gone on during the *pre-solution period*. But is this so? [The main point

brought out by these experimental data was that there was a definite positive correlation between the responses of the animals to the two stimuli (hypotheses if one wishes) in this new test problem and the relative number of times they had been previously reinforced on them. This result was entirely in accord with the writer's continuity theory. But it is not to be expected on the non-continuity view, for each 'hypothesis' had received an equal amount of training after being adopted. The differences exhibited in the preferences of the animals in the test problem must thus be ascribed to the *differential number of reinforcements received during the pre-solution period*. But according to the non-continuity theory these differential experiences should have had no effect. This fact is thus not only relevant to the non-continuity theory, but very embarrassing for it.

Similarly, the remainder of the experimental data showed that a very close relationship existed between the relative number of reinforcements (and after once being reinforced, the number of non-reinforcements) the two stimuli of any problem had previously had, and both the initial response and the rate of learning in the new problems. It was possible to predict which 'hypothesis,' if any, the subject might be expected to exhibit at the beginning of each new test problem and the relative speed with which each would be learned. In this connection, Krechevsky's statement that "There is no longer a pre-solution period for the 'test' problems in the sense that the animals might reasonably be expected to react to any other set of discriminanda such as spatial differentia, etc." (7, p. 127), is flatly contradicted by the facts. The data indicate that if the two stimuli to be discriminated in the new test problem happened to have received about the same number of reinforcements in previous, earlier problems, the subject would respond in purely chance fashion so far as the cue aspects were concerned, and would exhibit a position preference (hypothesis) or some other *systematic* spatial response, e.g., perseveration response. [When one of the stimuli had previously received many more reinforcements than the other, no such spatial hypotheses would be exhibited, but,

depending on the amount of difference in the number of previous reinforcements, would respond predominantly to the stimulus previously reinforced most. The scatter diagrams of these data indicated a continuous function, not a discontinuous one.

V

Haire (2) has recently carried out an experiment under the direction of Krechevsky which is supposed to furnish still further support for the non-continuity (Lashley) theory and yet another disproof of the continuity (particularly the writer's) theory. By altering the jumping stand of the Lashley apparatus so as to give the animal two separate promontories instead of the same central position from which to jump, it was found that a marked speeding up in learning was attained. According to Haire, the explanation of this result was that the change in apparatus design provided a clearer spatial articulation which in some unexplained way aided the rat to discard and avoid more readily interfering spatial hypotheses.

The writer's version of the continuity theory was then examined by Haire. He was unable to see how it could explain the facts; ergo as usual, it could not possibly do so. Haire's reasoning was based upon the statement of the writer that "The difference between the excitatory strengths of the cue stimuli must reach a certain minimum before the animal will respond consistently to the positive stimulus" (16, p. 432), plus the further assumption, taken on his own initiative, to the effect that in the single and double platform groups "the difference between the excitatory strengths of the cue stimuli are equal at the moment of 'learning' since they both display the same behavior characteristic (*i.e.*, 90 per cent or better performance)" (2, p. 88). Apparently Haire believed that the latter of the two above assumptions followed necessarily and logically from the first. Such is not the case, however, for the size of the difference between the excitatory tendencies of the positive and negative cue aspects of the stimulus situation necessary for consistent response to one of them (*i.e.*,

completed learning) *depends upon the presence of any systematic or variable differences among the excitatory tendencies of other aspects of the stimulus situation which are allied in their action with the cue aspects.*

As has been brought out in the writer's previous articles (14, 15), a greater difference between the excitatory strengths of the positive and negative cue would be required for the attainment of learning if there is a difference in the excitatory strengths of the position stimuli than if no such difference is present. Similarly, to the degree that variable stimulus factors are operative in the situation, now favoring (allied with) the positive aspect and now the negative aspect, a greater difference between the excitatory strengths of the cue stimuli would be required. The presence of such differences between the other stimulus aspects in the situation necessitates the building up of a sufficiently great difference between the cue aspects so that the stimulus complex of which the positive cue is a member is always (or in 90 per cent of the trials, or whatever the learning criterion is) greater in strength than that containing the negative aspect. Accordingly, although it is accurate to say that at the attainment of the learning criterion the differences between the stimulus complex containing the positive cue and that containing the negative cue must be the same for the two groups of subjects, the differences between the excitatory strengths of the cue aspects themselves need not necessarily be the same.

The explanation of Haire's results according to the writer's interpretation, then, would begin with the assumption that a bigger difference between the excitatory strengths of the positive and negative cue aspects is necessary with the single platform for the reason that any original difference in the excitatory strengths of the two position stimuli, S_L and S_R , would tend to be broken down more slowly under this condition than with the divided platform. The derivation of the latter part of this statement is, in turn, based upon assumptions as to generalization of reinforcement and non-reinforcement effects similar to those employed by the writer in his article (16) on the discrimination of stimulus differences of

degree, *e.g.*, brightness and size. Thus the two position stimuli would be conceived of as members of a single spatial continuum or dimension. In the single platform, S_L and S_R would be close together, while in the double platform they would be farther apart. The effects of reinforcement and non-reinforcement of a response to one of these stimuli, it is assumed, would generalize more to the other in the case of the single than in the double platform. But it may be shown that the greater the degree of generalization the less would the difference between the excitatory strengths of the position stimuli be reduced per non-reinforcement.⁵ To restate more briefly, then, the slower learning of the single-platform group as compared with the dual-platform group is explained as due to the circumstance that the greater generalization of the training effects on the position stimuli in the single situation would make for slower reduction of differences in the excitatory strengths of the position stimuli which in turn, according to the theory, would require a bigger difference between the excitatory strengths of the cue stimuli and thus more trials.

A test of the adequacy of this explanation as opposed to the 'hypothesis' view would be nicely provided by comparing the learning of the single and double-platform groups in the converse discrimination problem, half of the subjects in each group now being run on the single and half on the double platform. The reversal learning of the two original groups according to the view supported by Haire presumably should be the same. But according to the theory proposed by the writer a greater difference between the positive and negative cue stimuli was developed in the original learning problem in the case of the single platform group than in the double platform group. Hence, it should take longer for the former group to learn the reversal problem.

⁵ This may be shown very simply by assuming hypothetical values, *e.g.*, that $S_L = 70$ and $S_R = 50$, and further that the decrement from non-reinforcement is 5 points, with generalization to S_R of 3 points in the single platform and 1 in the double. After 5 successive non-reinforcements of responses to S_L the hypothetical strengths would be as follows: $S_L = 45$, $S_R = 35$ for the single platform and $S_L = 45$, $S_R = 45$ in the case of the double platform.

VI

In conclusion, one further characteristic of the pre-solution responses of animals in learning situations needs clarification. That systematic response tendencies or 'hypotheses' occur in pre-learning periods is by now a well established experimental fact. In addition to Krechevsky's evidence from discrimination learning experiments with white rats, the writer has recently presented data showing similar phenomena in the solution of multiple choice problems by chimpanzees. And much earlier Hamilton (4) had revealed similar kinds of 'reaction tendencies' exhibited to varying degrees by several mammalian forms ranging from the white rat to man in the insoluble quadruple choice problem.

A major difference exists, however, as to interpretation. Krechevsky has interpreted the hypotheses as being insightful, or intelligent responses, revealing something quite different from a 'blind process of trial and error' adjustment. According to the writer's theory, on the contrary, these presolution phenomena appear to be a typical example of what has been described as trial and error learning while hypotheses are far from what he understands by the terms insightful and intelligent. Only *persistent* non-adaptive responses can attain the distinction of being hypotheses—for, in order to classify as a hypothesis, a response, although ineffective, must continue to be persisted in a certain minimum number of times. A maladaptive act which is speedily (intelligently?) abandoned cannot ever be a hypothesis.

Degrees of intelligence as revealed in learning problems would, from the writer's point of view, be indicated by two main characteristics: (1) the readiness and persistence with which an ineffective response is avoided, and (2) the readiness and persistence with which the appropriate response is made, once hit upon. In the writer's opinion differences in these two characteristics are continuous. High degrees of readiness and persistence in each case do not differ in kind from low degrees. What has been termed intelligent or insightful

learning in animals differs only in degree from blind or slow learning.⁶

REFERENCES

1. GULLIKSEN, H., & WOLFE, D. L. Theory of learning and transfer. *Psychometrika*, 1938, 3, 3, 127-251.
2. HAIRE, M. Some factors influencing repetitive errors in discrimination learning. *J. comp. Psychology*, 1939, 27, 79-91.
3. —. A note concerning McCulloch's discussion of discrimination habits. *PSYCHOL. REV.*, 1939, 46, 298-303.
4. HAMILTON, G. V. A study of trial and error reactions in mammals. *J. Animal Behavior Monogr.*, 1911, 1, 33-66.
5. KRECHEVSKY, I. 'Hypotheses' in rats. *PSYCHOL. REV.*, 1932, 39, 516-532.
6. —. A note concerning 'The nature of discrimination learning in animals.' *PSYCHOL. REV.*, 1937, 44, 97-103.
7. —. A study of the continuity of the problem-solving process. *PSYCHOL. REV.*, 1938, 45, 107-133.
8. LASHLEY, K. S. *Brain mechanisms and intelligence*. Chicago: University of Chicago Press, 1929.
9. —. The mechanism of vision. XV. Preliminary studies of the rat's capacity for detail vision. *J. gen. Psychol.*, 1938, 18, 123-193.
10. MAIER, N. R. F. Qualitative differences in the learning of rats in a discrimination situation. *J. comp. Psychol.*, 1939, 27, 2, 289-331.
11. MCCULLOCH, T. L. Comment on the formation of discrimination habits. *PSYCHOL. REV.*, 1939, 46, 75-85.
12. —. Reply to a note on discrimination habits. *PSYCHOL. REV.*, 1939, 46, 304-307.
13. —, & PRATT, J. G. A study of the pre-solution period in weight discrimination by white rats. *J. comp. Psychol.*, 1934, 18, 271-290.
14. SPENCE, K. W. The nature of discrimination learning in animals. *PSYCHOL. REV.*, 1936, 43, 427-449.
15. —. Analysis of the formation of visual discrimination habits in chimpanzee. *J. comp. Psychol.*, 1937, 23, 77-100.
16. —. The differential response in animals to stimuli varying within a single dimension. *PSYCHOL. REV.*, 1937, 44, 430-444.
17. —. Gradual versus sudden solution of discrimination problems by chimpanzees. *J. comp. Psychol.*, 1938, 25, 213-224.
18. —. Solution of multiple choice problems by chimpanzees. *Comp. Psychol. Monogr.*, 1939, 15, no. 3, serial no. 75.
19. WILEY, L. E., & WILEY, A. M. Studies in the learning function. *Psychometrika*, 1937, 2, 1-19, 107-120, 161-164.

⁶ The writer is not including here certain higher forms of adjustment such as are mediated by the complex symbolic mechanisms of man. He has reference to the usual type of learning situation employed with non-articulate, infra-human organisms, e.g., discrimination, problem box, maze, multiple choice situation, etc. Another manner of saying the same thing as above is that sudden jumps in the learning curves of these problems are different only in degree from the more gradual acquisitions.

[MS. received January 22, 1940]

A NEGLECTED FOURTH DIMENSION TO PSYCHOLOGICAL RESEARCH¹

BY J. F. DASHIELL

University of North Carolina

I. Surely it is safe to say that all queries psychological reduce to one: what are those factors that determine just how a person is going to think or act? It is to this that the 'why' type of question resolves itself. Why does Johnny purse out his lips when his sister lays hold of the fire engine with which he has been playing? Why does the absent-minded scientist forget most of the items on his grocery list? Why will the child be spoiled if spared the rod—or if not spared the rod, in case you are a 'progressivist'? Why in the spring does a young man's fancy take a turn for the worse? Why do members of learned societies subject themselves to vice-presidential addresses? All such questions turn out to be questions of—what are the determining factors in each case, or, in a more positivistic spirit, what are the variables with which each change of behavior is related in a dependent way?

If we view the field of psychological research from a high vantage point, we can see that the determinants with which investigators are occupied fall into four most general classes. Or, I may say, the human nature variables lie along four different dimensions.

The one that has easily had the major share of attention is that of the *present stimulus-situation*. It is easy to see why. Experimental attack on human behavior most readily and simply took the form of making this or that modification in a man's physical surroundings and then noting what change took place in him.

This, however, is but one dimension. The single, solitary stimulus-response event has its historical, or rather its biographical, predeterminants. What a person does is deter-

¹Address of the retiring vice-president of Section I (Psychology), A.A.A.S., Columbus, December 27, 1939.

mined in part by his own past. Previous experience in the form of *habits*, then, furnishes a second dimension to psychological description.

Psychologists are finding that the fetal organism does behave and even can learn. And their heredity experiments in animal psychology as well as their manipulations of environmental factors in child development, further attest their concern with *genetic* factors.

These, then, are the three dimensions in which experimentation has been most prone to delineate the psychological nature of *Homo sapiens*. Or, more accurately, perhaps, these are the three categories of factors that determine what he does: the present stimulus-situation, this modified by his past experience, and this in turn erected upon the substructure of his genetic constitution.

But there is a fourth dimension. In each case a man is *set* in a certain way, and what he then does is determined by this set. I wish we had a more imposing term for this dimension, one of Latin or Greek derivation. 'Expectancy,' 'attitude,' 'disposition,' 'posture,' 'determining tendency,' 'excitation background,' and the like—each fails to satisfy by reason of too specific connotations.

But if 'set' is a simple word, the phenomena it denotes are by no means simple—a major reason, no doubt, for their neglect. I should like at this point to mention a number of the occasions in psychological research on which some kinds of temporary predetermining conditions in the individual have loomed up as important and have had to be taken into account.

II. It seems that in the early years of experimentation in psychology the topic of 'attention' was given belated and rather offhand recognition. Like the discovery of the 'personal equation' by astronomers, the realization that there was some nigger-in-the-woodpile led to the isolation of 'attention' as the miscreant and then to its systematic analysis. Much in this same spirit, the sort of human phenomena with which we are here concerned have been incidental rather than primary subjects of inquiry. Let me merely remind you of some classic cases with which you will be familiar, I know.

In the early *reaction time* studies, it became apparent that the reactor's speed depended not only on the nature of the stimulus but also upon the use of a warning signal, and upon the precise interval elapsing between them. The reactor was easily caught off 'set.' Stemming from this was Wundt's complication experiment, in which the 'prior entry' of one or other of two disparate stimuli, sight or sound, depended upon the direction of the reactor's concentration.

There were instances in *psychophysics*. Among the so-called errors that intruded on that work were: the error of 'adaptation,' in which the subject displayed a tendency to continue inertly making the same response, and the error of 'expectation,' in which his knowledge that his response was bound to change led him to make the change prematurely. There was also the 'absolute impression' of which Martin and Müller ran afoul, which gradually became established in the subject, so that he came to judge a given stimulus in comparison not with its paired stimulus but with this absolute impression.

A more colorful instance comes from the history of the *word-association* method. Instruct your subject that his verbal responses are to be 'opposites,' say, or to be 'subordinates,' of the stimulus words, and his average time for giving them is considerably modified by this predetermining set.

About the same time there appeared to Kraepelin in his analyses of the *curve of work* a brief, sharp rise in efficiency which he attributed to *Anregung* or 'warming up,' then a more gradual improvement called *Gewöhnung* or 'adaptation': a quicker and a slower getting-set.

Then there was the *interference of habits* that Bergström found in his card-sorting. In the course of learning to deal to one arrangement of piles a person would develop a greater readiness to continue dealing to that same arrangement and a reduced readiness to deal to another arrangement; and a crucial thing was the time interval elapsing between the using of one habit and the using of the other.

III. In the last decade or two still other manifestations of set have turned out to be crucial.

More direct scrutiny was given to preparatory states in experiments by the Würzburg School, when they turned to examining into the non-sensory texture of the *thought processes*, and came face to face with *die Bewusstseinslagen*, or intangible conscious attitudes, with *die Aufgabe*, or the awareness-of-a-certain-problem, with *die Einstellung*, or the determining tendency to work persistently in the given direction.

In Hunter's experiment on *delayed reactions* (15) animals that had learned to go to a lighted doorway instead of dark ones to obtain food, could be held back for some time after the light was turned off and still go to the previously-lighted-but-now-dark correct door. Some of the animals depended upon a body-pointing maintained through the delay interval, but some there were that showed no such dependence, and, we must conclude, utilized some more implicit kind of set.

In experimental studies on *learning* it has long been axiomatic that the learner must be motivated. In animal cases this 'r'arin' to go' is a 'set' based upon some temporary physiological appetite, while in children and adults it takes more complex forms of temporary mobilizations of interests that are more derived in character—interests that serve to restrain the learner's efforts to certain channels. (Consider Judd's work in 1905, 20, and Book and Norvell's in 1922, 3,—to name but two from a rich literature.)

A most interesting form of temporary predetermination within the learning experiments was brought out in Krechevsky's notion of '*hypotheses*' (21). In a series of trials animals will show a consistent form of behavior (such as taking always the right-hand choices) until, this proving ineffective, they follow another consistent form (such as choosing always the lighter pathways), each reaction-type persisting for a while and dominating successive runs.

Learning as *conditioning* has in these latter years been found not to be so crassly simple as hasty readers of Pavlov and Bekhterev had once supposed; and among the complications of the too-simple story of S-R's and the so-called substitution of S for S or R for R, a most important one Schilder (31) and then Schlosberg (32) found to be the dis-

turbing role played by the human subject's attitude toward the procedure of being conditioned, the particular way in which he was set.

Experimentation upon how a person will *reproduce* in drawings *ambiguous forms* that have been briefly shown him, has brought to light several manners in which his attempted reproductions are led askew by ways in which he happens to have gotten set. This can be established by his thinking of some object of which the figure happens to remind him (Gibson, 13), or by hearing the experimenter name some such object (Carmichael *et al.*, 5); and in either case his attempts at reproducing the stimulus-figure are warped markedly in that direction. In fact, with a differently suggested set he will fail to recognize the same figure (Zangwill, 39).

An impressive finding that has furnished the pattern for a number of investigations of temporary predetermination is that of one of Lewin's students (Zeigarnik, 40) who found that people are more likely to recall activities which had been *interrupted* than activities which they had been allowed to finish up. It is thought that a tension of some sort is set up when the task is tackled which, if the worker be interrupted, subsides very slowly, and so makes for a favored readiness in recall. Even the memory process known technically as 'reminiscence' is receiving treatment in terms of this persisting stress (Martin, 24).

And this suggests one of the more dramatic episodes in thinking—one that though not yet under clean experimental control is of obvious point in this connection. I refer to those experiences of *inspiration* in *creative thinking* that come often after one has apparently forgotten the unsolved problem he has had on his mind. Scientists, poets, and inventors, are one in representing these happy solutions as often appearing in such a way as to indicate that some kind of orientation to the problem has been persisting through recreation and even sleep (9, 30).

These samples may give some impression of the all-pervasiveness of factors we call 'set.' They crop out as things to be dealt with in arranging experimental controls on

almost every hand. They lie along another dimension in which the description of human behavior must eventually be drawn. They furnish us an enticing vast field of problems for investigations. So then, let us inquire: in what directions is research particularly promising? What avenues are opened up?

IV. For one thing these phenomena make us curious as to their *physiological bases*. And some attempts have been made so to deal with them.

Several of the earlier authorities (for whom Ribot made an excellent spokesman) in their introspective analyses of the experiences of getting set or being set, were impressed by the occurrence of sensations of strain, of tension, at those times; and so it seems that a favorite way of regarding the matter has been to refer to *muscular postures and adjustments*. (This is in line with the tradition of the response psychology, the history of which was sketched in Langfeld's address before this Section I seven years ago, 22.)

One direction taken is that emphasizing *minimal movements*. A set to respond in a particular way, the greater (temporary) readiness with which a certain kind of behavior is elicited, bespeaks some nascent, some partial activation of the appropriate muscles. The sprinter awaiting the pistol signal is a lively example. Now, ever since Jastrow's experiments in 1892 (18) there have been some experimental evidences to support that, at least by implication. By action potential techniques, Max (25), Jacobson (16), and still more to the point, Davis (8), have presented evidences that at least in some conditions resembling set the muscles that are about to be involved or that would have been involved in overt activity are betraying a degree of tension. Moreover, by the use of delicate levers Freeman (10) has gotten evidences of minute actual movements under similar conditions.

A bold position has been taken by those (Dashiell, Freeman) who have made the distinction between the so-called '*phasic*' and '*postural*' types of muscular response explanatory of the 'set' type of phenomena (6, 11). According to this view the slow, longer-lasting, diffuse postural reactions serve as a substratum for the quick, brief, localized phasic reactions.

The distinction has enjoyed some favor in neurological quarters (Sherrington and others, 33); but there has been disagreement as to just what is the ultimate basis for the supposed differences of function. Does it lie in the opposition between the cerebro-spinal and the autonomic divisions of the nervous system, or in a difference in kinds of tissue in the muscle itself, or in a dual innervation over the efferent nerves, or in a difference in the brain centers involved, or in a difference between the sense organs themselves, some giving rise to impulses arousing the slow postural contractions, others, those arousing the phasic? ² Who knows?

A variant of the motor viewpoint is that represented in a well-known quotation from James:

The *sense-organs* and the bodily muscles which favor their exercise are *adjusted* most energetically in sensorial attention. . . . But there are good grounds for believing that even intellectual attention, attention to the *idea* of a sensible object, is also accompanied with some degree of excitement of the sense-organs to which the object appeals (17).

In contrast to these motor theories are those that look upon set as being on its physiological side a purely *central neural* phenomenon. Even the 'motor adjustment' of Müller and Schumann (28) (the 'motor attunement' of Myers, 29) was, in spite of its name, given a central explanation. And von Kries, in his theoretical study of *Einstellungen*, assumed them to be cerebral throughout (36).

This conservative position is well expressed, as we would have expected, by Titchener (35) in his free use of such terms as 'cortical set,' 'nervous bias,' and 'nervous predetermination.' Experimental defense of a central notion of set is now appearing, as in a study of Mowrer and others (26) on reaction times. A response to a tone is slower if the subject has been instructed to respond to either the tone or a light, whichever is presented, than if to the tone alone; and response to a light is slower if the light comes after a series of tones than if alternating with tones. The fact that the motor response is

² For bibliography references No. 6 and 11 may be consulted.

identical in all cases, it is held, argues for a non-motor variable—a neural set.

The question whether some form of the muscular or some form of the neural theory shall eventually prevail, is a challenge to be met by investigations in which the contributions of the psychologist will be crucial.

V. But to return to more strictly psychological questions! When a process, condition, or event of the natural world becomes scientifically important inevitably we seek to get it described quantitatively, to *measure* it, to determine in mathematical terms the conditions with which it varies. So here our inquiry naturally takes the form: what methods of measuring these phenomena are in prospect?

Some ten years ago a procedure was tried out by Bills and Brown (2) for measurement of the effects of what they called 'quantitative sets.' For instance, they put their subjects to tasks of varying known lengths. Their results showed that both the initial level of work and the rate at which they slowed down varied directly with the size of the task with which the subjects were confronted at the outset. Here, let us say, we can measure the degree of readiness in terms of the degree of speeding or slowing of the work.

Mowrer (27) is suggesting the feasibility of methods of measuring what he calls 'expectancy' which attack more directly the preparatory set that had been recognized only qualitatively in some classical experiments. For one: we are reminded that in *reaction time* experiments if the stimulus be repeatedly presented at a regular interval (12 seconds), a presentation of it after some other length of interval will bring out the response more slowly; and he shows that as these occasional irregular intervals vary more and more from the standard 12-second one, the reaction time is prolonged more and more. Consequently he is able to present us with a curve indicating that, following the stimulus, the readiness to repeat the response drops markedly, then mounts to a maximum at the standard interval, then slowly drops. Here, we see, the course of expectancy is mapped by the index of *speed* of reaction.

Analogous results had been obtained by Brown (4) using as his index the *magnitude* of the reaction. Rats shocked regularly at 12-second intervals would, when shocked at interpolated irregular intervals, respond with smaller jumps. From his data it is then possible to map the course of expectancy as a curve, much as in the preceding case. Similarly, under conditions of repetitive stimulation Hull has found that the magnitude of the conditioned GSR made to a stimulus given at a certain interval varied as the number of times that that particular time-interval had been used (*cf.* 27, pp. 18 f.).

For another possible method: Consider that in the study of brain waves the Berger rhythm shows its maximal amplitude and regularity when the individual is definitely relaxed and best of all in sleep, and that it is found absent or depressed in states called 'excited,' 'aroused,' 'very alert,' and the like. Does it seem unreasonable, Mowrer asks, to consider expectancy to be the negative correlate of the *alpha wave*, and, therefore, a phenomenon the fluctuations of which permit of direct objective registration and measurement?

Again, he thinks that the '*time error*' in psychophysical research, which I mentioned as the '*adaptation error*' among the early examples of set, may turn out to be a tool of measurement. The lifting of a given weight seems to establish a set for again lifting a weight of that same mass, so that judgment of the actual weight next to be lifted is somewhat warped. It would appear, then, that by careful pairings of weights the precise strength of this *Einstellung*, as it has been called, can be estimated; and, of course, the curve of its lapsing with time.

One can recall other and even simpler-appearing quantitative attacks. For instance, it is one of the stock-in-trade demonstrations to compare the time taken to complete skeleton words that are *grouped* quite miscellaneous with that taken when they are grouped under specific heads (34).

Again, it is easy to compare accurately the time taken to complete a hundred examples in adding, subtracting, multiplying, and dividing, when done in happenstance order, and the time taken to do equivalent examples in a grouped order: 25 additions, then 25 subtractions, and so on (7).

With these examples before us, the prospect seems vast for indefinite extensions of the quantitative approach to many other classical cases. It is feasible to consider projects for mapping changes of amount and of set-under-scrutiny in a particular experiment, doing this indirectly by measuring and if possible graduating the changes in the conditions inducing the set, and measuring the changes in the consequent behavior. Then we will be studying the nature of the set (in psychological terms), rather than merely the relation between the set and reaction time, the set and the learning-rate, or similar indirect questions.

It may be asked, when one has applied more quantified methods to the direct and intensive smoking out of the set factors here, there and elsewhere, will they not be just so many isolated measurements related each to its own setting only? *This* is the persistent and insistent problem. Sometimes it may recede into the background as we work on particular things, as the determining tendency in a train of associations, or the pianist's warming up of a given key in which he is to play, or the increased fertility of one's ideas when he finds himself before a group. Yet the greater work of correlation and generalization and definition makes its inevitable demands. After all what *is* organic or mental set?

VI. Can the phenomena be theoretically structured? What I have just presented are thumb-nail sketches of a dozen and more very different occasions in experimental research when some recognition of this fourth dimension of psychological description has had to be granted. It is time that we tried to envisage and to *formulate* these phenomena in a *more general way* (38, 37, 12).

A contrast between specific occupations and the set phenomena has come out interestingly in MacFarland's tests of the effects of high altitude (23). The reports of mountaineers (14) that under such conditions they find it difficult to make quick shifts from one mental task to another and a tendency to repeat the same words or movements over and over, and the similar claims of war-time laboratories at sea

level providing reduced oxygen pressure (1), were both confirmed with aviators.

It is tempting here to look upon set and shift of set as referring to functions different in essential nature from particular acts and occupations. To illustrate: it is one thing to be able to name colors on a card at top speed, another to work at a switchboard throwing off lights as fast as they come on in chance sequence; but it looks like an ability of a very different order to be able to change quickly from the one task to the other. Can the former be 'habits' or 'skills,' say, and the latter 'versatility' or 'mental agility' or 'freedom from load'?

But this engaging notion has gotten little factual support. In the North Carolina laboratory some years ago we found that a variety of tasks could be finished more quickly, and improvement in them appeared more quickly, if each be finished before the next is taken up, than if they be taken in mixed order. But we found no evidence of independence or uniqueness of set; for we found that the rapid workers were also rapid shifters. More thoroughly Jersild (19) went into the question with a variety of tasks in calculation, controlled association, opposites, color naming, and the like; and he concluded that the difference between the activity in a given task and that of shifting between tasks was a difference not of kind but merely of degree. Each activity called for some adaptation and adjustment, and the more complicated and multiple the activity, the broader the set. Shall we conclude that the ability to shift from one task to another is a function of learning, and that the ease or difficulty in shifting is an expression of the degree to which the habit of next higher level has become organized?

Set, then, is in part a character of the organization of particular 'behaviors' rather than something in addition to them.

But further, sets are hierarchical! Immediately a whole nest of queries suggest themselves, queries that offer vistas of opportunity. Let us see some concrete samples. A child—according to the Lewin technique—is interrupted before he can finish a task, and his likelihood of recalling that task later

is enhanced. The set for that task is somehow operative. But the child is also set to follow the experimenter's instructions; or, he is set to play with whatever is placed before him. Or, consider the reaction-time subject again, who is primed not only for reacting after the constant foreperiod interval but also for reacting to the specific stimulus-signal, for reacting at all, for coöperating in the experiment at all, and so on. Clearly here we have sets of varying degrees of generality or inclusiveness, the more inclusive supporting the less.

But are the sets of which a person is seized and possessed always teamed up? By no means! Antagonisms have played some part in psychological history. Witness experiments of Binet, Paulhan, and others, on disparate simultaneous activities, such as adding on paper while reciting the alphabet backward. Witness, too, the disastrous conflict appearing when a dog can neither solve his discrimination problem nor avoid that shock-to-come. In the North Carolina laboratory we have under way an adaptation of the distraction experiment, in which we want to find out the relative disturbing-value on different complexities of work, of unorganized sounds, of familiar musical airs long-continued, and of airs abruptly interrupting each other; and thus observe the interference of different levels of temporary set upon different levels of ready-organized habits.

We see, then, that there are many kinds of sets! Some are of wider scope, are more general, than others. Sometimes they coöperate and reinforce, sometimes they antagonize and inhibit each other. So that a complete picture of how John Doe is set at a given instant would be a composite picture indeed!

VII. We might well be abashed by the assortment of names that have been applied by this man and that: 'attitude,' 'readiness,' 'orientation,' 'expectancy,' 'determining tendency,' 'predisposition,' 'Einstellung,' 'temporary preparation,' 'organic disposition,' 'excitation background,' 'anticipatory reaction,' 'preparatory adjustment,' 'postural response,' or just the simple but accurate Anglo-Saxon word, 'set.' But let others debate the names; we are more con-

cerned with the phenomena. Can we, as we survey the many different sorts, discover any points in common?

For one, we note in all the cases an implication of a *differential readiness*. One movement, word, idea, or what you will, is more ready to appear than are others that have the same advantage so far as past experience goes. This may take a quantitative form in the increased quickness or intensity of a response; or it may appear qualitatively in the manner in which the readier response displaces and supersedes others. By response, now—and this is important!—I do not always mean something specific for it may be a whole class of responses (as in controlled word-association), which by being more warmed-up renders more available any specific response within its family (as, any ‘opposite’ or any ‘subordinate’ word).

Once we have perceived that readiness is at the heart of the phenomena we are considering, an avenue of research is opened up. *What, it can be asked, are the conditions that increase the readiness of a given habit or ability?* The raw data we must always start with are, of course, the actual performances. How much these are indicators of a ‘true’ ability, how much of propitious conditions of present stimulation, and how much of a temporary readiness on the individual’s part, can be determined only by investigations. And so, I suggest, just as we try to isolate a ‘true’ ability by holding constant all other factors, so we can try to isolate the readiness variable, the availability, of a given activity. The optimal fore-period in reaction time work, the most effective conditions of motivating the learning of arithmetic, the social facilitation of co-workers—these come to mind as encouraging examples of what I mean.

Another mark! Canvassing all the cases I have cited we see that each condition has resulted from some form of *perseveration*, taken in the less technical sense of some persistence or inertia. Something in the way of a hang-over of the immediately preceding manner of responding, or a persisting after-image of a vanished stimulus, or a continuing verbal formulation, dominates the behavior of the succeeding

moments. Are we put to it to explain this lag, this perseveration? I think not. We shall find an enormous simplification in recognizing here, as in theoretical mechanics, that inertia is the thing to be taken for granted, and changes of course or state the things really calling for explanation. No longer do we ask, concerning a body flying through space, why does it continue on in a straight line instead of slowing down or 'dropping,' but rather, what extraneous conditions pull it out of that course? Analogously, there is little point in inquiring into the nature of set, as some force, agency, or function *per se*. Rather we should ask: what are the conditions that produce the changes and shifts? If a person is adding or is whistling 'Yankee Doodle,' it should cause us no wonder that he continues adding or continues whistling this tune, but the real question is—what diverts him, what makes him stop?

This perseveration, now, must not be conceived in too narrow a way. We have, for example, those interesting phenomena of Lewin's students who have demonstrated tensions that may persist quite *below* the *conscious* threshold.

And this reminds us of the *purposive* or goal-seeking character of some forms of set. But I see no need of a new classification here. The goal-seeking character of all behavior may be argued pro and con on its own points. And as for conscious purposes, while they do indeed emerge out of the 'expectancies' and 'predispositions' with which we are here dealing, the manner of this emergence is not particularly our concern in the present discussion; and we need only recognize the many degrees of persistence with which an idea can be entertained.

There is one rather frequent connotation of our term 'set' which I wish you to examine—its so-called *directing* character. This is prominent in many varieties of it, as in the reproduction of visible forms or as in the hypotheses of the actively seeking animal or human. But is it invariably to be found? Consider those studies in which the 'expectancy' or 'preparatory adjustment' is effective primarily or even solely in speeding up the simple voluntary reaction or the conditioned eye-wink or the dynamometer pull. In those cases when the way in which

a person is set does seem to exercise a directive control over how he then thinks or acts, I suggest that this is, logically speaking, a merely accidental character. It is an expression of the advantage that flows to those words, thoughts, or acts that are consistent with the ready-prepared state. To illustrate more concretely: in the delayed reaction, what bears the appearance of a picking-out is nothing more than a persisting orientation. Or again, when one is told that the form flashed before him represents a 'shovel,' this word arouses a number of habitual details in the drawing of shovels, each now becoming more ready and available. *The 'set' part of this is primarily these increased readinesses*, and only by consequence is there any selecting or directing. The directing function of set is, I submit, neither universal nor primary.

Finally, the attempt to arrive at some general characterization of the phenomena of set, bringing them under one head, may possibly take more than a theoretical form. Can the new powerful tool of factorial analysis be applied? The project, if it is to be inclusive and convincing, would reach gigantic proportions. We would need to devise tests for the 'set' character in each of the many diverse lines of experimental study that have been mentioned. We would have to quantify the appearance of this character in each instance. We would need then to apply the whole formidable battery to an adequate number of subjects to obtain intercorrelations in the degrees to which the character appeared. Would a factor or factors common to all come to the surface?

BIBLIOGRAPHY

1. BAGBY, E. The psychological effects of oxygen deprivation. *J. comp. Psychol.*, 1921, **1**, 97-113.
2. BILLS, A. G., & BROWN, C. The quantitative set. *J. exper. Psychol.*, 1929, **12**, 301-324.
3. BOOK, W. F., & NORVELL, L. The will to learn. *Ped. Sem.*, 1922, **29**, 305-362.
4. BROWN, J. S. A note on a temporal gradient of reinforcement. *J. exper. Psychol.*, 1939, **25**, 221-227.
5. CARMICHAEL, L., HOGAN, H. F., & WALTER, A. A. An experimental study of the effect of language on the reproduction of visually perceived form. *J. exper. Psychol.*, 1932, **15**, 73-86.
6. DASHIELL, J. F. *Fundamentals of objective psychology*. Boston: Houghton Mifflin, 1928.

7. —. *Fundamentals of general psychology*. Boston: Houghton Mifflin, 1937. Pp. 318 ff.
8. DAVIS, R. C. Set and muscular tension. *Ind. Univ. Science Series* (to appear).
9. DOWNEY, J. *Creative imagination*. New York: Harcourt, Brace, 1929.
10. FREEMAN, G. L. The spread of neuro-muscular activity during mental work. *J. gen. Psychol.*, 1931, 5, 479-94.
11. —. The postural substrate. *Psychol. Rev.*, 1938, 45, 324-334.
12. —. The problem of set. *Amer. J. Psychol.*, 1939, 52, 16-30.
13. GIBSON, J. J. The reproduction of visually perceived forms. *J. exper. Psychol.*, 1929, 12, 1-39.
14. HINGSTON, R. W. G. Physiological difficulties in the ascent of Mount Everest. *Geograph. Jour.*, 1925, 65, 1: 4-25.
15. HUNTER, W. S. The delayed reaction in animals and children. *Beh. Monogr.*, 1912, 2, No. 6.
16. JACOBSON, E. Electrophysiology of mental activities. *Amer. J. Psychol.*, 1932, 44, 677-94.
17. JAMES, W. *Principles of psychology*. New York: Holt, 1890, I, p. 434.
18. JASTROW, J. *Pop. Sci. Mo.*, 1892, 40, 743-750, 41, 636-643.
19. JERSILD, A. T. Mental set and shift. *Arch. Psychol., N. Y.*, 1927, No. 89.
20. JUDD, C. H. Practice without knowledge of results. *Psychol. Monogr.*, 1905, 7, 185-198.
21. KRECHEVSKY, I. 'Hypothesis' vs. 'chance' in the presolution period in sensory discrimination learning; and The genesis of 'hypothesis' in rats. *Univ. Calif. Publ. Psychol.*, 1932, 6, Nos. 3 and 4.
22. LANGFELD, H. S. The historical development of response psychology. *Science*, 1933, 77, 243-250.
23. MACFARLAND, R. A. Psycho-physiological studies at high altitude in the Andes. *J. comp. Psychol.*, 1937, 23, 191-225, 227-258, 24, 147-188, 189-220.
24. MARTIN, J. R. Reminiscence and Gestalt theory. *Psychol. Monogr.* (to appear).
25. MAX, L. W. An experimental study of the motor theory of consciousness. *J. comp. Psychol.*, 1935, 19, 469-486.
26. MOWRER, O. H., RAYMAN, N. N., & BLISS, E. L. Preparatory set (expectancy)—an experimental demonstration of its 'central' locus. *J. exper. Psychol.*, 1940, 26, 357-372.
27. MOWRER, O. H. Preparatory set (expectancy)—Some methods of measurement. *Psychol. Monogr.*, 1940, 52, No. 233.
28. MÜLLER, G. E., & SCHUMANN, F. Ueber d. psychologischen Grundlagen der Vergleichung gehobener Gewichte. *Pflüg. Arch. ges. Physiol.*, 1889, 45, 37-112.
29. MYERS, C. S. *Text-book of experimental psychology*. Cambridge, Eng.: Univ. Press, 2nd ed., 1911.
30. PLATT, W., & BAKER, R. A. The relation of the scientific 'hunch' to research. *J. chem. Educ.*, 1931, 8, pt. 2, 1969-2002.
31. SCHILDER, P. Conditioned reflexes. *Arch. Neur. Psychiat.*, Chicago, 1929, 22, 425-443.
32. SCHLOSBERG, H. An investigation of certain factors related to ease of conditioning. *J. gen. Psychol.*, 1932, 7, 328-342.
33. SHERRINGTON, C. S. Postural activity in muscle and nerve. *Brain*, 1915, 38, 191-234.

34. STARCH, D. *Experiments and exercises in educational psychology*. New York: Macmillan, 3rd ed., 1930. Pp. 182-186.
35. TITCHENER, E. B. *Text-book of psychology*. New York: Macmillan, 1910.
36. VON KRIES, J. Über die Natur gewisser mit den psychischen Vorgängen verknüpfter Gehirnzustände. *Z. Psychol. u. Physiol. d. Sinnesorg.*, 1895, 8, 1-33.
37. WOODWORTH, R. S. Situation-and-goal set. *Amer. J. Psychol.*, 1937, 50, 130-140.
38. YOUNG, P. T. The phenomena of organic set. *PSYCHOL. REV.*, 1925, 32, 472-478.
39. ZANGWILL, O. L. A study of the significance of attitude in recognition. *Brit. J. Psychol.*, 1937, 28, 12-17.
40. ZEIGARNIK, B. Ueber das Behalten von erledigten und unerledigten Handlungen. *Psychol. Forsch.*, 1927, 9, 1-85.

[MS. received February 21, 1940]

THE POSTULATE OF COMMON CONTENT¹

BY KNIGHT DUNLAP

University of California at Los Angeles

A few months ago, a student came to me with a question about a question. The basic question was this: "Can it be proved or disproved that two persons can sense the same color?" The student's query was: "Is that a significant question, or is it meaningless?" I told the student that the question seemed to me decidedly significant, although in the form in which it was put an adequate answer could not be given, since either *yes* or *no* would be false in implication. The importance of the question, I went on to say, is that it brings under scrutiny the whole foundation of psychology and its methods. "What," I asked, "made you think the question might not be significant?" "Well," he replied, "I asked the question of the professor in my philosophy class, and he refused to answer it, and said it could not even be discussed, because it is without meaning."

I do not know what 'meaning' the philosopher attached to his statement that the question was without 'meaning.' I might suspect that the covert meaning was that the discussion of the question would take him out of the safe shallows of his philosophical system, and that he did not intend to drown publicly. As for myself, I would rather drown in public than be afraid to try to swim, so I shall take the student's question as the text.

Most psychologists today, like some philosophers, are loath to strike out in the deeper and saltier philosophical waters, and so the problems of the relations between the several conceptual fields or levels with which psychology deals are not much discussed. The old controversies over the several sorts of parallelism, interactionism, and other *isms* of

¹ This paper and the following papers by Albert G. Balz and H. M. Johnson were read at a Symposium on the Philosophy of Science on March 23, 1940, at the meeting of the Southern Society for Philosophy and Psychology.

historic note, have been soft-pedalled. Such splashings as modern psychologists attempt are mostly done in the shallows of Gestalt, and various other modern *isms* and *ologies*, where the turbidity of the waters conceals the lack of depth.

The philosophical timidity and discouragement of the present era, however, are in large part due to the obvious sterility of the older disputes, in which the categories involved were so vaguely and shiftily conceived, and the terms employed were so slippery, that any protagonist could easily prove, to his own satisfaction, that his opponents' positions were untenable, with no effect whatever on the opponents' opinions. Often, the theory demolished was one that the opponent did not really hold. I would not say that the battles which raged on the 'Body-Mind' front a generation and more ago were fights against wind-mills. Rather, they seem, in perspective from the present, to have been bouts between wind-mills and straw men.

The several problems confused in the 'Body-Mind' controversies have not been solved, so far as the majority of psychologists are concerned. They have simply been dodged; and the results of the evasions are evident in the chaos in modern psychology, with its endless stream of 'schools,' and the real threat of complete dissolution of psychology in the near future. The problems must be discussed; and as a basis for my presentation, I shall enumerate the principal categories.²

1. *Substance*, of a *trans-subjective* sort. (The terms *substance* and *trans-subjective* are not assumed here to have definite significance.) Perhaps Aristotle's *nous*, and Philo's *logos* belong here. This category appears sometimes to have been called *soul*, but I wouldn't be positive as to this application of the term of so many meanings. The category has, however, been sometimes identified with *God*.

2. The *ego*, or *subject* of experience. I think Kant's 'Transcendental Unity of Apperception' belongs here; but like all Kant's concepts, this may be interpreted in other ways.

² In employing the term *category* in this way, I apologize to Aristotle, since my use of the term is not exactly the same as his. This will probably not confuse my readers, since few psychologists today are familiar with Aristotle's conception, and those who are will have no difficulty with mine.

The *thumos* of the Homeric Greeks certainly is in this category. The *nous* of Anaxagoras and Democritus has functions which place it here, although in other respects it is identified with other categories. The *psyche* of Plato, and possibly of other late Pythagoreans, was certainly a subject of experience, although Plato assumed that the body, as well as the *psyche*, was a subject. The *daimon* of the early Pythagoreans, which was involved in the miscalled doctrine of 'metempsychosis,' belongs here as an accessory subject of experience, although not the ego of a man. Plato's *psyche* is, I think, the first concept to which the name 'soul' can be applied without confusion, for if this concept appeared in Pythagoreanism before Plato, we have no direct evidence therefor. Aristotle, of course, rejected the *psyche* of Plato; and although in some of Aristotle's writings his *psyche* appears as the ego, this interpretation is not consistent with Aristotle on the whole.

3. The system, or world of *consciousness*, in one of the two most frequent meanings of that term. The items in this category may be called 'acts,' 'processes,' or 'states' of being aware, or of being conscious; provided these terms (act, process, state) are not taken too strictly. This category is implied, although often not discussed, by all philosophers, as well as in science and in popular ideologies. Malebranche, for example, implies it in addition to his two worlds; and Spinoza assumes it implicitly in addition to his dual-aspect substance. James recognized it, but deemed it no business of psychology. The term *mind* in one of its many meanings is applied occasionally to this category.

4. *Content* of consciousness; or the collection or system of such contents. Items such as 'sensations,' 'feelings,' and 'images' are included. The term 'consciousness' is applied to this category by psychologists more often than to Category 3. This category seems to have been unknown to the Homeric Greeks, and to have been first suggested by Democritus, although the suggestion was probably unintentional. Aristotle ignored it completely; and its important development was through the Stoics and Epicureans, from whom modern philosophy and psychology took it over without essential

change. The *ideas* of Malebranche and Spinoza belong to this category exclusively, although Descartes is ambiguous. In the so-called 'introspectionalist' psychology, this category is theoretically the exclusive field for psychological study. 'Gestalt' appears to have been originally the pattern of items of content.

5. The *external world*, sometimes called the 'physical' world. Items in this category may be considered to be perceptible, or to be imperceptible although closely resembling items in the world of content. The world of *appearance* of Parmenides, the physical world of Anaxagoras, the extended world of Descartes and Malebranche, and the extension-aspect of Spinoza's substance, are in this category, which must not be confused with that of matter as included in the next category. The Greek *hule* has this significance most often, although sometimes designating other categories.

6. The *material world* of matter and energy. The term 'matter' is here used in the sense of Democritus' atoms, and the electrons, ions, etc. of modern physics. While the conception may have arisen in Ionian philosophy before Democritus (leaving the mythical Leucippus out of consideration), we have no clear evidence for this. Aristotle ignored it, which is our best evidence that Democritus invented it.

7. Ultimate external reality; *trans-objective substance*. This is dimly hinted at in the Ionian philosophy, and seems to have been conceived by various philosophers, ancient and modern. Spinoza's God, conceived not under one of its attributes, but as the ground for them, might be put here. This category must not be confused with Category 6.

The foregoing list is not presented as exhaustive, but as including the categories which have been involved in an important way in the philosophical discussions of the past. That there are hierarchies of subordinate classes which might be called 'sub-categories,' is obvious. The expected criticism, that the list is redundant—that there are not, really, so many orders of reality—may be valid, but is irrelevant. I have not invented these categories. They have arisen in the philosophizing of great philosophers of the past. We cannot exclude

any of the categories from consideration, for each of them has been emphasized, and distinguished from other categories, by one philosopher or another, one school or another. That one philosopher or school has distinguished what others have confused, and confused what others have distinguished, makes the study of all of them unavoidable. If we find eventually that one or more categories can be deleted or identified with others, such deletions and/or combinations can readily be made. To proceed arbitrarily with deletions and omissions, is to prejudice the issue of the investigation, and plunge into the redolent murk in which the former discussions of the 'Body-Mind' problem sank.

In the discussions of the 'Body-Mind' problem after Malebranche and Spinoza these categories were thoroughly confused, although it appears that Malebranche and Spinoza were not confused. As for Geulincx, all I can infer from my struggle with his abominable Latin is that he has been grievously misrepresented by conventional historians of philosophy, most of whom, I suspect, never read him.

For Malebranche, for example, 'body' seems clearly to be in Category 5. For less skillful writers, it may be in 4, 5, or 6, or slide from one to the other as the needs of the argument vary. The term 'mind' has also been a sliding term, on which authors have, in the main, skidded from 3 to 4 and back again. That 'body' has not skidded at times into 7, and 'mind' into 2 or even 1, would be a risky thing to say. We may ignore these latter caroms for present purposes, since philosophers who deal with 1 and/or 7 are commonly damned as 'mystics'; and psychologists who are concerned with 2 are causes of eyebrow lifting.

The 'parallelists' for the most part seem to have concentrated their efforts on the attempt to disprove causal relations (interaction) between 4 and 5, *e.g.*, Malebranche and Spinoza; or between 4 and 6, with occasional skids into denial of interaction between 3 and 6, or 5 and 6. The interactionists, on the other hand, have argued mainly for interaction between 4 and 5, with inadvertent arguments for interaction between 3 and 4.

If we consider the term 'body,' in a restricted sense, namely, as indicating the human body, we find that in many expositions the body, and in particular that part of the body called the brain, figures as the perceptible body, which is either in Category 4 or in Category 5; it being often difficult to determine which is meant. In other expositions, the body is assumed to be an affair of electrons, atoms, ions, or what have you: presumptively, therefore, in Category 6. One of the stock arguments of the parallelists of fifty years ago, that a 'sensation' cannot possibly be an intermediary between two chemical processes considered as changes in molecular or atomic configuration, might indeed be a valid point. To carry this conclusion over to the impossibility of interaction between the brain, as a perceptible object, and a 'sensation' as a perceptible item, is, however, a flouting of the accepted principles of logic. Similarly, the assumption that there is a causal relation between mental objects (4) and 'physical' objects (5) even if admitted, cannot be skidded into the assumption that there is interaction between mental objects (4) and matter (6); or between consciousness (3) and physical objects (5), without violation of the laws of logic.

I have used three terms which now require our careful attention: *sensation*, *consciousness*, and *cause*. Their consideration involves the consideration of introspection also. A lot of trouble has come from the defining of one of these terms in one way, and then using it in other ways, or from developing the concept vaguely without definition, with certain (or uncertain) implications, which makes the skidding into other implications easy.

I. 'Sensation' as the word has been used, and is used at present, has three meanings.

A. Sensation as an abstract term, is used to designate what we call, more concretely, the *sensing* of something. The *seeing* of red is sensation. Sensation here is a term referring to items of consciousness (3).

B. As a concrete substantive, the term is used to designate a sense-datum, or sense object. In this usage, 'sensation' is obviously in Category 4.

C. In a usage which psychologists formally deplore, 'sensation' means a process in the nervous system, for which we prefer the terms 'nerve process' or 'nerve current,' or in restricted reference, 'brain process.' No matter how the psychologist protests, this usage is so firmly fixed in the usages of physicists and physiologists, that we seldom succeed in weaning undergraduates away from it; and many graduate students never get over the habit. Although sensation in this usage most commonly is in Category 5, it is sometimes intended to be in Category 6. This usage is confusing, but perhaps has not been as damaging as the confusion between sensation (3) and sensation (4).

II. The abstract noun *consciousness* has, at present, two meanings, both of which are different from the original meaning of the term in English.

A. Consciousness (3). In popular usage, except in so far as popular usage has been corrupted by Freudian confusions, the term represents abstractly that which is expressed more concretely by the forms of the verb *to be conscious*, and of the verb *to be aware*. Various items in this consciousness (3) are listed as: sensing, imagining; feeling; thinking; etc.

B. Consciousness (4). In wide psychological usage, 'consciousness' is a concrete noun designating the content of which one is aware. Consciousness (4), in other words, is a sort of mental stuff, which Wundt frankly called, distributively, *psychic objects*. Although the American psychology derived from Wundt avoided the term 'object,' and denied that consciousness (4) was a stuff, preferring to call it a 'process' or 'processes,' it was obviously confused by consideration of consciousness (3), which, although theoretically excluded from consciousness (4), constantly cast in its shadow. Further, this psychology was never able to decide just what the 'process' was a process in. For a process is, of course, a change in something capable of changing; and the introspectionalists were never willing to admit that the changes were in the brain (which would have solved their problem) or that they were changes in a mental stuff (which also would have solved the problem).

The importance of the distinction between consciousness (3) and consciousness (4) was, indeed, recognized from time to time, but psychologists generally have preferred to define consciousness in Category 4, using, in theory at least, certain other terms such as *observation* and *introspection* for consciousness (3). Skidding into usage 3, of course, was frequent. James admitted the importance of 'awareness' (consciousness 3) of the outer world, along with introspection (observation of consciousness 4), but denied somewhat inconsistently, that this 'awareness' is any business of psychology. Titchener eventually agreed with James, but the inconsistency of this declaration with his theoretical position was even more glaring.

Others have recognized the distinction more definitely. The late Professor Frost at one time proposed a new term, 'to consciensize,' for consciousness (3). My colleague Dr. Donald Williams, has more recently proposed another substitute term. I have, for a long time, used the term 'consciousness' only in sense 3, employing the term *content* where I mean content, or employing the conventional subsidiary terms such as *sensum*, and *sense datum*; but I despair of any of our efforts bringing order out of the psychological chaos.

That this confusion is responsible for much of the difficulty which students have in grasping the principles of psychology, is evident. How can students grasp them, when their instructors and text-book writers do not? Behaviorism was founded on the confusion; for the behaviorist, in denying consciousness (4), could never free himself from the notion that he had to deny consciousness (3). The present absurdities of the doctrine of the 'unconscious' can be cleared away only by first clearing up the confusion of consciousness (3) with consciousness (4). Theoretically, German has a terminology through which the confusion might be obviated, but actually, the same confusion is found in German writers.

The danger in the attempt to use a term which has a fixed popular usage, with new definition opposed to that usage, is well illustrated by the confusion of the two meanings of 'consciousness.' However carefully the psychologist defines

consciousness as 4, he inevitably skids, at critical moments, into usage 3.

It is a curious fact that, although the subordinate term *sensation* partakes fully of the ambiguity of the superordinate term *consciousness*, the term *image* is used by psychologists always in Category 4. The 'image' is always regarded as content; and being aware of an image is never called an 'image,' but is called *observing an image*, although the ambiguity of the word 'consciousness' breaks through; for the observing an image is often called 'consciousness,' or awareness of the image.

III. The word 'introspection' has had, in psychology and outside, three important meanings, all in Category 3.

A. Awareness of awareness.—Introspection in this sense is synonymous with consciousness in the original usage of that term. Having been profoundly influenced by Stout in my youth, I turned my later attack on introspection on the term in this sense. I suspected that although 'introspection' was defined by the Titchener school in a way which formally excluded Stout's meaning, the latter was not entirely out of the picture, but was influential in determining, illicitly, certain features of the introspectionalist creed. My attack, at any rate, led to the clear repudiation, by Titchener, of this sort of introspection; which clarification caused him further trouble.

B. Introspection, as formally conceived by introspectionalists, was awareness of content, that is to say, consciousness (3) of consciousness (4). A formal distinction was made between this and awareness of external reality (5), which was called observation, but not introspection. Since, according to Titchener's parallelistic postulates, one can observe only his consciousness (4), never the external world directly, it would seem that this distinction between introspection and external observation could hardly have been drawn except for confusion of introspection as defined by Titchener with introspection as defined by Stout. However, the distinction between introspection, as the awareness of consciousness (4), and observation of external reality (5) was made the basis of Titchener's 'stimulus error.' The stimulus error was, in brief,

committed by observing stimuli (5) when one should be observing (introspecting) content, *i.e.*, consciousness (4). At any rate, the inconsistency of the stimulus-error assumption with his basic philosophy became apparent to Titchener, and he found no satisfactory way out of it, and would have been glad to have the stimulus error forgotten. Some of his pupils, however, would not let it be forgotten; and I suspect that this was one of the seeds of the discord between Titchener and his disciples, which led him, late in life, to complain that the ingrates had all turned against him.

C. A third usage of the term introspection, still within Category 3, is for consciousness (3) of a limited range of contents (4); or of 'physical items' (5). An old usage, now abandoned, made introspection include the processes of imagining and feeling, that is to say, awareness of 'images' (if any) and awareness of affects. Sometimes 'thinking,' as a broader concept than 'imagining,' was included. In any case, the usage amounted to a distinction between sense-perception considered as the function of the 'external senses' and introspection considered as the function of the 'internal sense.' At the other extreme, the term has been used to designate consciousness (3) of the organism itself (5), of whatever is going on in *soma* and *viscera* (*i.e.*, within the limits of the skin). Between this usage, and the discarded one, there are various other concepts, the clarification of which is of importance in attacking the further problems of 'feeling and emotion,' and of maladjustment. The much abused *feeling* has many meanings in popular and scientific usages, and the clearing up of its appalling confusions has definite bearing, ultimately, on the question I took as my text.

IV. The word *cause* is another fugitive term, for which there are many shades of meaning. It is no secret that attempts to define cause away from its popular meanings have had no great success, and there are reasons. Causation, whatever it is, might occur (or operate), theoretically, in any of the categories. Something called 'causation' seems to have been assumed in Categories 1, 2, 5, 6, and 7. Whether in 3 and 4, is not clear. The consideration of causation within a cate-

gory, however, does not seem to have been deemed as important as that of causation between categories. One form of parallelism denies causation within Category 4 (which perhaps no one ever assumed); but makes its especial point in denying causation between 4 and 5, or between 4 and 6. I do not know that the question of causation between 3 and 4 has ever been ventilated. I suppose that some of the modern mathematicians posing as philosophers have implicitly assumed causal relations between 7 and 6, but whether they have understood the implication or not, I would not want to say. Causation between 2 and 3 has been assumed, and unintentionally, perhaps, between 2 and 4.

This spreading of causation over a number of categories may well arouse our suspicion. It may be that one cannot apply the term to different categories and different inter-categorical relationships, in one and the same sense. The concept of the transfer of energy might apply in Category 6. If we apply it to 5, is the meaning of 'energy' the same as for 6? It is possible that the conflicting concepts of causation are concepts developed for different categories. There may even be causes of several different kinds in one category, as Aristotle held, and different causes in other categories, as Aristotle also seemed to hold.

I think it is safe to assume that the concept of causation arose in Category 5, long before the conceptions of 4 or 6 arose. This seems to be the only category in which the concept could arise, since it appears to have preceded the emergence of philosophy. In the Ionian philosophy the notion of causation between 5 and 7 is latent, or vaguely developed with the notion that 7 is just a more primitive 5. Parmenides seems to have been distressed by the concept of inter-categorical causality, and it emerges as a definite conception first in Democritus. For Aristotle, of course, Category 4 did not exist and causation occurs in Category 5 exclusively, except for the special kind of causation in Category 1, in which the *nous* has causal relations only with itself. Aristotle's *psyche* is in Category 5, and therefore involved in the general nexus of causation.

That the concept of causation had its origin in Category 5, and was extended to other categories and to intercategory relations not only is borne out by historical considerations, but is in harmony with the fact that the concept, as popularly held today, derives its real significance from the sequences of phenomena in Category 5. The significance of the concept in this category, I submit, is fundamental. The extension of the same concept to other categories, if possible, requires justification. If concepts of different sorts are developed in other categories, and intercategory, their right to the name 'causation' should be challenged.

The nature of causation in Category 5 is, I think, adequately indicated by Mill. The notion of succession or other concomitance, so nearly invariable that the laws of the concomitance can be discerned and made the basis of prediction, is, without question, the basic notion of causation in this category. Further notions of causation as transfer of energy in Category 6 may be useful, but should not be confused with the fundamental notion of causation. Animistic notions of causation as mystic energy-transfers from 2 to 5, with 3 as intermediary, are also analogies drawn from actual causation as observed. Notions of causation as *mana* are more primitive than either of these other derivative notions, and may be considered as merely crude ways of indicating causation as described by Mill. All derivative and analogical concepts of causation need to be justified by reference to the fundamental concept from which they are drawn; or, if they cannot be justified as transformations of the basal, observable causal sequences, they need to be labeled as having no relation to this causation.

For psychology, in its own field, the fundamental concept of causation should suffice, since psychology deals with the observable world (4 or 5), and the world of consciousness (3), and not with a mathematical substitute (6) therefor. Since the psychologist must make excursions into the field of physics, just as he does into history and political science, he may need to deal, in those fields, with causations of other sorts; but must not confuse them with the concepts appropriate to common

praxis and to psychology. If one asserts that the psychologist deals with Category 4 instead of 5, the same considerations obtain; for, on this assumption, all that primitive man dealt with was 4, without witting it, and the causal sequences are sequences in that world. Of causal relations between 3 and 4, the conclusions would be different from those concerned with causation bridging 3 and 5. For, in psychology as in common praxis, it is assumed that there are invariable sequences *from* patterns in 5, *to* conscious processes in 3. But, if 4 be assumed instead of 5, then the sequences are reversed; although in neither case would a translation of causation into the causation of physics be intelligible.

Psychology, it appears from my viewpoint, accepts the postulate of the continuity of nature (5) which is common to the sciences. It further adds a postulate of its own, namely that there is a limited continuity between Category 5 and Category 3 (or between 4 and 3, if 5 be rejected). In this latter case, however, 6 is left hanging in the air, as a mathematical framework for a world which does not exist. The assumption of a limited 4, in addition to 5, is of course consistent with our other assumptions, if shown to be necessary. For, a further postulate shared by all the sciences is essential to psychology, the postulate, namely, that scientific hypotheses must conform to the principle of economy. If the assumption of a limited world 4, in addition to world 5, is a simpler hypothesis than the assumption of 5 without 4, then it is a valid hypothesis. From these postulates results the further postulate that hypotheses which are economical can be validated or corrected by experiment and/or controlled observation. These postulates, with one more which I will formulate immediately, cover the concept of causality for psychology. If physics on the one hand, and mystical philosophy on the other, need additional kinds of causation, that is their affair.

I come now to the final postulate, adopted practically in our procedure in the psychological laboratory, as well as in the praxis and doxis of everyday life. This is the postulate of common sensibles, *sensa*, or *sense-data*. It is the assumption

that there are *sensa* in world 5, which can be sensed by all persons whose sense organs are adequate to them. We do not, of course, assume that a blind man can see colors, or that a partially deaf man can hear all sounds that other men hear.

Let us note carefully the implications of this postulate, and particularly, what it does not imply.

I. It does not imply that any content of thinking can be common, or shared by two persons. The question as to the possible community of thought content is not prejudiced either way by this postulate.

II. It does not imply that all perceived or perceptible items are, or can be, common or shared. Nor that when two men share a common *sensum*, each man will perceive it as fully as the other. Differences in the intensity and extensity of the *sensum*, for example, may be found.

III. It does not imply that the experiences (consciousness 3) are common, or shared. In fact, the postulate might well be formulated to exclude this possibility. One man's seeing is not another man's seeing, nor is one man's hearing another's hearing.

IV. It implies a certain relationship between Categories 5 and 4. In so far as items of content are common (and we may include here 'potentially common'), they belong to the 'physical' or external world. Items which are not common must be either in world 4 alone, or in world 5 alone.

V. It implies that matter, *qua* matter, since it is not perceptible, is either in Category 6, or else in the thought-content part of world 4. The postulate does not favor either alternative.

VI. It implies that certain features of *sensa* (whether they are considered to be in Category 4 or Category 5 does not matter for the moment) are *absolute*. That they may be perceived, or not perceived; but if perceived are identical for any two men, or for the same man at different times.

VII. It makes logically possible causal relations *from* world 5 *to* world 3; and thus conforms to the principle of economy by making the universal assumption of common praxis and *doxis* identical with that of psychology, and further by har-

monizing the procedure in psychological laboratories with psychological postulates.

VIII. The enveloping implication is that it is a primary business or concern of psychology to determine the *extent* to which one man perceives the same data as other men; and through these determinations to arrive at the absolute features of *sensa*. That psychology, through the ages, has been working persistently on just this problem is no news to any reader.

Our procedure in psychology, it is true, has not been deduced from this postulate, but has developed from its implicit assumption. We demonstrate this every time we say: "Come here and see this"; or "Listen to this." If we try to explain away this implied postulate, by saying that all we mean is: "Turn your eyes into such and such a position," "Walk in such and such a direction," "Make such and such verbal responses," we cannot take our subterfuges very seriously. All of the processes just described are either imperceptible complexes in worlds 6 or 7, or perceptible complexes in worlds 4 or 5. To say that two persons, observing a man walking, or pressing a key, do not in any part see the same man or the same actions, would make our experimental procedures mere absurdities. The inconsistency is parallel to that of the early behaviorists, who deemed that a patient either could not perceive, or that his perception was so 'subjective' as to be of no importance; while holding that the behaviorist's own perceptions of the subject's actions were highly reliable, and that another man checking on him could see the same things, or at least see the same smoked paper records.

One of the arguments against community of *sensa* which has long been popular is drawn from the alleged facts of abnormal color vision. We are told that a color-blind man and a normal man, looking at the same piece of colored paper, will see different colors. This objection backfires violently, however; for if colors were completely private contents we would have no way of knowing that two men see them differently. The objector then falls back on his postulate of privacy, and dares anyone to dislodge him. We might leave him there to stew in his own juice, but we may point out to him that our postu-

late not only gives a ground for his assertion of differences in colors seen, but goes further and enables us to determine what the differences are, and what is the common factor: that the psychological postulate justifies the research which has been done on color vision, while his postulate nullifies itself.

The determination of the precise primary or absolute colors has long been a goal for psychologists, physiologists and physicists; and at least demonstrates the unanimous acceptance of our postulate by scientists who deal with this problem. That the problem has not been completely solved is not to be counted against the quest or the postulate. It is worthy of note that the absolute features of *sensa* of taste and of audition have been pretty well determined, although no progress has been made with odors.

Returning now to my puzzled student, I can tell him with assurance: Your question is not adequately formulated. We cannot prove the proposition in the minor way in which we may prove a minor proposition. So it would be false to answer *yes*; and at least as false to answer *no*. We can say, however, that two men can see the same color, under proper conditions; and this statement is as true as any statement that the psychologist can make about human beings, for it is a particularization of one of the postulates on which psychology is founded, and is validated as all postulates must be validated, namely, by showing that it fits, in an economical way, not only the further postulates of uniformity of nature and the existence of laws of causal sequence, but also the practical procedures of the psychologist, and of all men unsophisticated by superficial philosophy.

[MS. received February 16, 1940]

CONCERNING THE SUBJECT-MATTER OF PSYCHOLOGY

BY ALBERT G. A. BALZ

University of Virginia

During a long age there existed a tradition, virtually unchallenged, according to which Philosophy and Psychology were related in a peculiarly intimate way. This tradition was expressed in organized graduate study, in academic organization, and in many other ways. In more recent years, the tradition has lost some of its strength. Here and there it is frankly challenged. In other quarters the tradition has persisted in form but has little vitality. The change has doubtless been due to many causes. Whatever the causes, there has emerged a new attitude, reflected in Philosophy as well as in Psychology. Philosophy, it appears to be thought, is related to Psychology as it is related to any and every other science; but Philosophy has no special significance for Psychology, and Psychology has no contribution of exceptional interest for Philosophy. Along with this goes the thought that the historical relations of Philosophy and Psychology were just a matter of history, and of a rather deplorable history to boot. Psychology is a science, with problems and a subject-matter of its own. Its proper affiliations are with the other sciences of man and of other living things. Psychology has gained dignity as an independent science with the decay of the tradition. Philosophy too has gained by the decay in that it can free itself from the confusion of philosophical analysis and psychological explanation. Something like this indicates a widely prevailing point of view.

I do not doubt that the change has been accompanied by gain. But I am inclined to think that there has been loss as well as gain. Having been reared in the older tradition, there persists the conviction that in some sense Philosophy and Psychology have by right an intimacy of relation that makes

each of especial significance for the other. The conviction prompts effort to find grounds that will justify it. Effort, however, is baffled at the outset. It is notorious that philosophers do not agree as to what Philosophy is. It is less notorious, perhaps, but equally true that there is little agreement among psychologists as to the fundamental standpoint, the scope, and the subject-matter of their science. It is perhaps hopeless to attempt clarification as to the nature of Philosophy—philosophers will doubtless wrangle about this till the end of time and of philosophers. With respect to a science, however, one has a right to entertain hope. I am prompted, therefore, to raise this question: What is the subject-matter of Psychology? That I can answer this question is far beyond my claim. If preliminary investigation will lead to competent discussion of this problem, Psychology will achieve more effective organization. In turn, the relations of Philosophy and Psychology can be more profitably re-explored. Perhaps the tradition of their intimacy of relation will receive revision and again serve as guide to cooperation in inquiry.

I propose, then, to discuss the question: What is the subject-matter of Psychology? Psychology, I assume, is a science, related to other sciences; as a science, it claims that measure of independence which befits the dignity of a science. In the measure in which a science properly claims independence, whether of other sciences or of Philosophy, in that measure it must claim jurisdiction over a correspondingly distinctive subject-matter. Deny the latter, and then the former must be denied. The claim to be an independent science is meaningless unless it involves sovereignty over a subject-matter. The question concerning the subject-matter of Psychology is then a legitimate question.

Two ways of meeting—or rather, of avoiding—the issue come to mind. We could say: There are psychologists; psychologists conduct inquiries; their results are what is meant by Psychology; and the facts, whatever they are, with which these inquiries are concerned, constitute the subject-matter of Psychology. This dismissal of the question has a certain practical force. The description implied would indicate much

concerning the status of the science yesterday and today. The claim to the dignity of an independent science, however, could scarcely be satisfied with such an answer.

The question might be dismissed on loftier grounds. The question, it could be urged, must be dismissed for the reason that it is unanswerable. Each science is in an incomplete, and unfinished, state. Although independent of one another, yet the various sciences are intricately related. They in part complete one another; they complement each other; yet they repel one another, and insist upon their respective sovereignties in the League of Sciences. Their territorial jurisdictions are none too clearly determined. On the one hand, each science involves a more or less tightly knit web of fundamental conceptions. These conceptions form the frame of reference for its subject-matter. The inquiries arising with a given science fall within this frame of reference. The frame of reference, in general, and the inquiries, more specifically, indicate what data are thought to be relevant for the inquiries, and accordingly suggests more or less exactly the subject-matter of this science. Doubtless the ideas constituting the frame of reference, as well as the secondary ideas within the science, are subject to continual revision and reorganization. The story of such alterations would be the history of this science, and would also be the history of the claims to subject-matter for the particular science. It is certainly the case that changes in the science prompt new explorations and annexations of existential territory, while efforts to colonize the new territory prompts revisions in the science. This intricacy in the relations of the 'science' to its 'subject-matter' is made even more complicated by the interactions of the various sciences with one another. It is unnecessary to unravel these intricacies in detail. If they are recognized, it is difficult to resist the conclusion that in such conditions we cannot answer our question, nor, for that matter, demarcate the subject-matter of any science.

In a word, we are led to a paradox. Ability to indicate the subject-matter of a science depends upon the state of this science, and more or less remotely, upon the state of the other

sciences. Psychology, like the other sciences, is incomplete. Hence we cannot determine its subject-matter. Only if and when a science shall have been completed, perfected, carried out to the limit, could we answer the question: What is the subject-matter of this science? Indeed, if the independence of a science is not unqualifiedly absolute, but relative; if the interrelations of the sciences are genuinely significant; then, we are tempted to conclude, no single science could be brought to perfection unless all were so completed. All the more then is the question concerning subject-matter unanswerable.

At this point a sheerly speculative question confronts us. Must we not go further and say: If and when the sciences are completed, will not the plurality of the sciences be dissolved in a single vast Science of all Existence? If, in their imperfection, they are interdependent and inter-related, does this not convincingly suggest the ideal of a single all-comprehensive Science, a Science that, having mastered all existence, would have sovereignty over a single vast subject-matter? In this ideal state of affairs, any and every question concerning subject-matter would be answered. No questions concerning the subject-matter of this or that science would even arise, for such questions appear only because there is a plurality of sciences and the sciences are incomplete.

A scientific conscientious objector might well urge that the question be dismissed. The discussion, he might say, has become uncontrollably speculative. We really do not know what the independencies and dependencies, the attractions and repulsions, of the various sciences may mean in some undefinable ultimate sense. At best, the scientist might concede, the notion of a single perfected all-comprehensive Science of Existence can serve as nothing more than a *limiting* idea, a vague indication of the ideal impetus that underlies the undertakings of inquiry. It cannot function in specific scientific inquiries.

The philosopher, I admit, must concede that the notion of a perfected all-comprehensive Science of Existence must remain, for the work of scientific inquiry, a limiting concept. Scientific inquiry is contextual. The set of fundamental ideas

constituting the frame of reference of a science defines the widest context within which the inquiries of this science proceed. Further restrictions of problems within this wider context are accompanied by similar restrictions with regard to relevancy of existential data. Subject-matter, then, is contextual. The limiting concept, accordingly, lies beyond such contexts and restrictions. So far it is methodologically irrelevant with regard to scientific inquiry. But granting all of this, it does not follow that the limiting concept is irrelevant to reflection *upon* science, nor that we are justified in dismissing as insoluble and unreal a question concerning the subject-matter of a science, Psychology or any other.

Indeed, it may be contended that the idea of a single completed Science of Existence is something more than a mere limiting concept. It is rather a projection of the essential faith of the scientist as such, a faith that the scientist seeks to resolve upwards into knowledge. It is not within the scope of this paper to defend this use of the term 'faith.' It must suffice to urge that the limiting concept is the representation of the ideal of scientific inquiry and a reflection of the actual facts concerning the sciences. Despite their plurality, the relations, dependencies, interactions, the borrowings and lendings, of the sciences point towards an ideal of unification. On the one hand, this ideal implies the coherent organization of the various frames of reference into a sort of superior frame of reference. On the other, the ideal points to the conviction that the whole of Existence forms a single subject-matter, a total realm of things.¹

The latter conviction, if it reflect the actual facts concerning the sciences and their procedures, implies that for the sciences Existence is not radically and irretrievably pluralistic. I shall use the term 'Nature' to stand for all that exists—for

¹ No attempt is made, in the present case, to define the meaning of this notion of totality. It may well be urged that the idea of the unity of Existence can be given concreteness of meaning only in the degree in which the sciences provide the materials for reflection. Historically, concepts such as that of the 'uniformity of nature,' of nature as a realm of causal law, whether they are acceptable or not, are indications of how reflection upon science projects the notion of the unity of Existence as at once an 'assumption' and an 'ideal' or limiting conception of the sciences.

all of the things that a total inventory of the contents of time and space would enumerate, for all events with respect to which location in space and time, or in time alone, appears necessary in understanding the events.² Now, if what has been described as a limiting notion for the sciences is also an ideal implied by the facts of scientific inquiry, then it would seem clear that the work of the sciences implies a conviction of the unity of Nature. Ideally, Nature is a single ultimate subject-matter for science. The plurality of the sciences implies some kind of partitioning of Nature; but this partitioning, this plurality of subject-matters, is contextual, functional, but not final. There may be, so to speak, island-universes of things or events, utterly disconnected from the materials with which any of our sciences deal. Such an admission is, however, an empty gesture. By hypothesis, such an island-universe would be unknown and unknowable. The admission is accordingly not merely an empty gesture for the time being, but it is intrinsically and of necessity empty. It is meaningless for science. Contrariwise, then, the inter-connectedness of things, the unity of Nature, is implied in the very activity of the scientist. And this is a gesture, not intrinsically empty, but merely irrelevant to contextually limited scientific inquiries. It reflects the nature of the scientific enterprise and its horizontal ideal.

Nature or Existence, then, is for the sciences a single subject-matter. In what sense, if this be granted, can the subject-matter of a particular science be indicated? As a first step, let me make the bald statement: The whole of Existence, the totality of Nature, is in some sense the subject-

²The following device may serve to make clear the sense in which the term 'Nature' is used. The theologian—it seems safe to assume—would accept this distinction: Whatever may be said to exist, to be a thing or an event, and at the same time cannot be said to be the Creator, is a creature, a created thing, a part of Creation. In this sense, if anything exist, and yet lies outside or beyond Nature or Creation, then it is God, or the Creator. If there are angels, and if angels are created beings or 'receive' their existence, then angels are 'natural' beings, are in 'Creation' or 'Nature,' and angelology would be a natural science. If there be any trait, property, or characteristic that every created thing shares with every other, then this trait would be shared by the angels as well as by stars, plants, and animals. It will be noted that human soul-substances, in Descartes' doctrine, take the place of the angels.

matter for each and every science. Each existential science claims *all* things for itself. The contention may be stated in another way. Theoretically, if connectedness runs throughout Nature, any event in Nature may have some measure of relevance to the problems which are the especial concern of this or of that science. The solar system, let us say, is within the jurisdiction of Physics, of Chemistry, and of Astronomy. But surely it is also within the realm of Biology. In so far as the immediate environmental conditions depend, for their existence and for their intelligibility, upon more remote environmental conditions, the solar system and its history are relevant for some, if not for every, biological inquiry. This situation finds expression in the inter-dependencies of the sciences, in their ability to profit by the advances of each science.³

This, of course, is but another way of expressing the point that the distinctions of subject-matters is relative, not final, contextual and functional, not absolute. But it is equally apparent that the relativity of the distinctions of subject-matters is not arbitrary. It cannot be the case that the distinctions are merely foisted upon an indifferent Nature. Were this the case, the sciences themselves would be without anchorage in fact. The problem of defining the subject-matter of any particular science falls within two general conditions: On the one hand, it must reckon with the condition that any 'fact' whatever is directly or indirectly relevant to each and every science. Accordingly, we cannot think of Nature as

³ The position taken in no wise conflicts with the restrictions characteristic of scientific inquiries as such. Concreteness, contextual limitation, of problem or of data relevant to problem, is relative, not absolute. For one biological problem, the solar system may lie beyond the field of relevancy, or be implicated only indirectly; for another, the solar system lies within the field, but the galaxy does not. In the sense indicated, the tentacles of scientific inquiry tend to extend indefinitely throughout the range of nature. It is indeed this fact that gives point to the possibility of two sciences having a common fund of ideas, however small, and to the employment of each other's findings. Moreover, this relationship spans even the distinction between the so-called descriptive and so-called normative sciences. Biologist and psychologist as such may be indifferent to the ethical character of a given social practice—say, cannibalism or monogamy. They are not indifferent to it as factual material falling within their subject-matters.

geographically parcelled out among a number of sovereignties. On the other hand, it is Existence itself that prompts restrictions of inquiry, with the resultant plurality of frames of reference. Distinctions of subject-matter, arising with the proliferation of a plurality of sciences, reflect not merely the necessities of thought but also the complexity of things.

Recognizing these conditions, it would appear that two resources are available. On the one hand, there is the history of the particular science itself, the fund of ideas with the revisions, deletions, and additions that define the movement of the science. On the other hand, on the basis of all that we know, guided by all of the various sciences, we must seek aspects of things, factors of fact (to borrow a phrase of Whitehead's), that by virtue of kinship suggest relevance to a special type of inquiry. The problem of defining the subject-matter of a special science is practically soluble only when the history of the science itself is viewed as an effort to determine its jurisdiction, and in turn Nature is inspected in order to find conditions that prompt the movement of the science.

With this as background, we may make a first approximation with respect to the subject-matter of Psychology.⁴

If Nature be viewed as a vast collection of events, we may distinguish two sets within the collection. One set may be described as follows: The occurrence of these events is *not* conditioned upon the existence and activity of living organisms. For them, living things are neither a necessary nor a sufficient cause. The other set may be defined as follows: They are events for which living organisms are either (1) necessary events if they are not sufficient causes, or are (2) necessary and also sufficient causes. Were all human life to cease on this planet, then for a visiting Martian scientist, human biology would be a bio-archæology. The set of events, then, with respect to which human organisms are a necessary, or a necessary and sufficient, cause constitute the subject-matter of human biology in a first approximation. To this must be added, of course, the organization and functioning of the

⁴ For the purposes of this paper, Psychology is taken as meaning 'human psychology.'

organism. The total set of events likewise embraces those which may be regarded as occurring 'within' the organism, however direct or indirect their connection with events 'outside' the organism. The organism is thus a central point from which the relatedness of things is viewed.⁵

The subject-matter of (human) Psychology, it would appear, falls within and bears special relations to that of human Biology. If the former, however, cannot be differentiated from the latter, then Psychology will not be a relatively independent science, associated with the biological sciences, but will be nothing more than a bundle of rather special biological inquiries.⁶ The history of science, from Aristotle to the present, certainly suggests an intimacy of relation between Psychology and Biology. But from the rational soul doctrine of Aristotle, through the Cartesian dualism of man and animals with the duality of man himself, to the present, there is

⁵ Professor Weedon, to whom I am greatly indebted for suggestion and criticism, has made me aware that this first approximation raises grave questions, for the philosophy of science if not for science. The analysis so far attributes—verbally at least—a substantial status to the organism, as if it were a fixed thing in a stream of events. Even scientific language may suggest that the organism is a sort of container within which events occur. Streams of events impinge upon it; these streams pass through it while the organism, from its own resources, contributes to the stream; thus the organism transmits back to the environment consequences dependent upon the organism. Refinement of analysis, however, would subject such statements to serious criticism. The problem of isolating the organism from the sea of events arises. From one standpoint, whether or no this be definitive, the organism tends to be dissolved into a set of events within a veritable sea of events. This drive of analysis has been manifest in the history of Psychology. Thus, for Descartes, the situation for Psychology was defined by a substantial soul with its 'states.' With parallelism, the substance concept yields to the concept of series. James' view of the stream of consciousness seems to be an effort to dissolve 'substance' into 'states' while retaining something of the unity and continuity implied in the substance notion. Perhaps something analogous occurs within the biological sciences. For the relatively limited purposes of the present paper, however, it seems unnecessary to attempt the refinements and re-statements this would require. The improved statement, in any case, must conserve the point involved in the more common-sense statement, *i.e.*, that the primary subject-matter of human Biology are those events or aspects of events that would never come-to-be if human organisms did not exist.

⁶ It may be that, in some remote sense, (human) Biology and Psychology may flow together to form a single science, a Psycho-biology. There are speculative philosophical positions that would imply this. But such possibilities—if they are possibilities—are far removed from the actualities.

apparent an effort to support the independence of Psychology. Pursuit of our theme is then historically justified.

Let us assume, then, that human Biology deals with events with regard to which man, his structure, his functions, his actions and reactions, are at least a necessary condition. Psychology is concerned with this same set of events, or with a part of them, or finally with 'aspects' of some or all of these events. On the indicated basis, no part of Psychology's subject-matter could lie *outside* of, and be totally irrelevant to, the field of human Biology. Even extra-sensory perceptions, if they occur, are events for the occurrence of which the human being is a necessary condition.

Granted that this determination of the subject-matter of human Biology defines the field within which we must find the subject-matter of Psychology, how can we go further? Our search is for something, not indeed totally irrelevant to Biology, but relevant only in an indirect sense. Stated in another manner, our search is for some means of differentiating the aspects of events or factors of fact that can enter biological inquiry only as results of a science of Psychology. Irrelevance, no less than relevance, of existential data is determined by the growth of a science in and through its own history. As indicated before, our search for clues may properly begin with the history of science itself. If Biology neglects something, it may be due in part to accident, but in part it may be due to the selective functioning and the historical accumulations of the science itself. What clues, then, are provided by history?

The first clue is afforded by a curious predicament in which the psychologist finds himself to be. In some quite special sense, the inquiring psychologist, the man who is both a human being and a psychologist, is a part of, or a necessary condition of, his own subject-matter. Stated otherwise: There are events, for which presumably there can be a scientific treatment, of such a character that the inquirer conditions both the inquiry and the data of the inquiry. In a special sense, Psychology appears to be irretrievably anthropocentric and egocentric. There are existential happenings, or

aspects of these, having this peculiarity that they are *directly* accessible only to the individual. The phrase, the privacy of consciousness, whatever else it may rightly or wrongly mean, at least indicates this character. I do not contend that Psychology should be defined as the science of consciousness, or that conscious states are the subject-matter of this science. That definitions of this sort should have been proffered in the course of history may serve as clues is sufficient.

A distinction must be made between the case of the psychologist and that of other scientists. All inquiry as such may be regarded as anthropocentric and egocentric. All inquiry is carried on by human beings. Every scientist is a biological human specimen. Human beings, including scientists, are materials for a number of sciences. For the anatomist, it matters not at all what cadaver be available. In principle, for the anatomist, his own body is material as satisfactory as any other. It is practically impossible, at least beyond narrow limits, for the anatomist to dissect his own body in the pursuit of his inquiries. The difficulty, however, is merely practical—it is rooted not in the nature of the subject-matter as such, but in the accidental fact that one and the same thing, the human being, is both an inquirer and a specimen of the material with respect to which inquiry is instituted. Within limits, scientists do sometimes serve as their own guinea-pigs. Beyond these limits, they proceed on a basis of substitution—of guinea-pigs for men, cadavers for their own bodies, other observers' reports in place of their own observations, and so on in a multitude of ways. Many practical problems of the control of inquiry are involved in the accidental anthropocentrism of all inquiry.

With the psychologist, the case is different, if we can trust the revelation of Psychology's own history. With respect to *some* if not all of the events within his jurisdiction, what I have called anthropocentrism is not accidental, but essential; it pertains to the subject-matter itself. Substitution is impossible. The psychologist's subject may report his 'inner' experience, and so a bridge of communication is thrown from privacy to privacy. In the end, however, the definitive data

for some inquiries must be provided by the inquirer himself. Such inquiries lead back to the psychologist's own direct apprehension. Inquiries involving this peculiarity of fact may form a large or a small part of Psychology—the question is here unimportant. Events with the characteristic indicated may be denied admission to the field of Psychology by some who are *de facto* psychologists. I do not wish to quarrel about words. Nor do I presume to dictate what inquiries scientists should pursue. I am not, however, prepared to admit that there are events in nature with respect to which no scientific knowledge is possible. If the events now referred to fall within the jurisdiction of any existing science, I do not know what this can be, on historical grounds, save the science of Psychology.

History affords at least one other clue. Psychology is first of all concerned with man, whatever may be said of its extension to the animals. The specific and differentiating traits of man must be central for the frames of reference, both of human Biology and of Psychology. History records many efforts to define these traits. The efforts frequently have reflected ethical, religious, political, and even metaphysical interests. Whether the efforts were successful or not is really beside the point. What is important is that they show a rough convergence. Aristotle pointed to the rational soul, and the consequences of its concomitance in man with the vegetative and animal souls. St. Thomas, employing the notion of an intelligible substance apt for unification with the human body, provided an elaboration of the Aristotelean position. Descartes, concerned primarily with a metaphysics for science, stumbled into a conclusion that made of man something less than an angel but extraordinarily more than an animal. Our modern Aristotle, John Dewey, brings the record down to date, and may serve to provide the clue. He describes as 'extraordinary' the "differences that mark off the activities and achievements of human beings from those of other biological forms." Dewey recognizes that these extraordinary differences are crucially important for an inquiry concerning Logic. Surely for Psychology they are not less

important. Now Dewey's own mode of accounting for the facts is indicated in this statement: "The conception to be developed . . . is that the development of language (in its widest sense) out of prior biological activities is, in its connection with wider cultural forces, the key to this transformation."⁷

Aristotle, Thomas, Descartes, and Dewey have, so to speak, a common denominator for their views. The use of symbols, it is suggested, serves as a clue. The use of symbols, the making and use of symbols, let us say, is peculiarly characteristic of man as man. Symbols suggest resources within man primarily if not uniquely characteristic of man. If it should appear that symbol-using and making point to dimensions of human biological facts that are of only indirect concern for Biology, then a clue to the differentiation of Psychology from Biology may be provided.

The field of possible symbols, and of their uses, seems illimitable. Any natural thing or event can be used to stand for something else. It could be argued that nature is a totality of just such a character that anything in it is a natural sign or symbol of other things. Perhaps this explains why 'superstition' as well as 'science' pivots upon the connectedness of things. For present purposes, and for Psychology as Psychology, it is irrelevant to distinguish between the scientific employment of a fact as a natural sign of something else and the unscientific, superstitious or irrational employment. The heavenly bodies are read in horoscopes, and nebular motion in the red shift in the spectrum. Whatever may be the difference between Astrology and Astronomy, both are concerned with taking one thing provided by nature as a sign of other things. In addition to this natural symbolism, and its uses, man provides himself with artificial symbols constructed from materials afforded by nature, including his own body. With respect to artificial symbols, there are limitations set by the natural properties of things and also by considerations of utility and convenience. In any case, the field of symbols is virtually illimitable.

⁷ J. Dewey, *Logic, the theory of inquiry*. New York: Henry Holt, 1938. Pp. 43-44.

If Psychology be peculiarly concerned with the resources specifically characteristic of man as man, with the consequences of the exploitation of these resources, then the universe of symbols objectively represents the field of Psychology. This statement points in two directions. On the one hand, it gives meaning to the position that for every science the totality of nature is directly or indirectly subject-matter. A thing, say a traffic-light, a gesture, the spectrum, is within the field of several sciences. The physicist passes from the spectrum to the star—with precautions he owes to advice from physiologist and psychologist. But the psychologist passes from the spectrum to the physicist, and ultimately from the physicist to himself. Stones and men both dwell within the universe—but with special consequences for the universe because of men. Men, since they are physical things, may interact with other things in very much the same fashion as is characteristic of the stone. But, in addition, in, through, or because of man, man interacts with things and things interact with one another in a fashion not presumably occurring with the stone.⁸ Things are made to mean one another or are taken as if they meant one another. Infinite possibilities of substitution, of one thing for another, therewith accrues to the totality of nature. Illimitable diagnostic possibilities are open to inquiry, and all the sciences tend to flow together in a sort of cosmic medical science. In such a way, all the sciences become auxiliaries of Psychology.

This clue, if it be a clue, tempts one to define Psychology as the science of appearance. But qualification is necessary. The term 'appearance' can be taken in the sense that things affect or influence one another—in the sense in which the sun warms the earth. But appearance in this meaning is rather subject-matter for sciences other than Psychology—perhaps primarily for Physics.⁹ It is rather appearance as symbol and

⁸ With respect to the plants and animals other than man, I venture no opinion. The question is obviously dependent upon what should be intended by 'animal' psychology.

⁹ Perhaps this should extend even to such matters as sense-organs, so that in one meaning the study of sense belongs to Biology rather than Psychology, and in another meaning to Psychology rather than Biology.

the reference of symbol, with the consequent substitutional possibilities, that would seem to be the field of Psychology. With this, the second clue seems to converge with the first clue. If a thing, whether natural or the product of human making, has the status of being a symbol,¹⁰ then it must be examined in two contexts of inquiry. On the one hand, the thing must be considered with respect to its properties as an existing thing. Broadly viewed, the thing in this sense falls within the scope of sciences other than Psychology. On the other hand, the status of the thing as a symbol demands another context of inquiry. Under what conditions does a thing come to have this additional status? Stated negatively, the question is this: What are the conditions that, if absent, would imply the lack of status as a symbol? It is one matter for a thing to exist; it is another for it both to exist and *also* to serve as a symbol. We may point to society and the cultural tradition, to education, habit, but in the end we point to the properties and powers of man as man. Thus we are driven back to reckon again with the fact that Psychology is anthropocentrically organized with regard to its very subject-matter.

I do not presume to have defined the subject-matter of Psychology. I have merely attempted to indicate two clues to a mystery that only those more competent than I can unravel. It would be an unexpected success, indeed, if the two clues were accepted as veritably clues. For what it is worth—and that is very little—I would state my conviction that greater attention by psychologists to the problem of their science's subject-matter would enhance the dignity of their science and reinforce its claim of independence. Moreover, in my conviction, such efforts would tend to reinstate, in altered form, the tradition of intimacy and productive interaction between Philosophy and Psychology. Having been trained in that tradition, it is something of a shock to perceive its destruction. The dignity, independence, and

¹⁰ This statement should be understood as falling within the context set by psychological inquiry. For logic and metaphysics, the status of being or serving as symbols must be considered in a very different context.

immanent authority of Psychology should not be imperiled by the dissipation and absorption of Psychology within other sciences. It would be baffling to learn that Psychology does not exist and that there are no psychologists. It would be appalling to conclude that our psychological associates are perhaps just anatomists, whetting their knives, convinced that the only useful philosopher is a dead one; or perhaps just biologists, puzzled that creatures so unfitted for survival as philosophers disappointingly do survive; or just physiologists reducing our dazzling speculations to the malfunctioning of unmentionable glands.

PRE-EXPERIMENTAL ASSUMPTIONS AS DETERMINERS OF EXPERIMENTAL RESULTS

BY H. M. JOHNSON

Tulane University

There is an old story, which may not be wholly true, about some professors in the University of Pisa. One morning about 350 years ago, as they filed past the leaning tower on their way from prayers, there was standing on its top a young instructor, called Galileo Galilei. They had recently argued with him, and they disliked him exceedingly for his bad manners. He called to them to notice what was about to happen. He then dropped together two cannon balls, one weighing one pound, the other ten pounds. Lo! Both balls struck the ground at once!

The professors were sorry to see this, and although the young instructor called a second time, they would not look again.

Why would they not look? Because, to each of them, Aristotle (1) had proved from his own postulates, and by a process which they did not question, that if two heavy bodies fall through the same medium, their speeds will be proportional to their weights. Therefore, what they had just witnessed *could* not have happened; therefore it *had* not happened.

It is often asserted, by uncritical authors, that Galileo's experiment eliminated from the affairs of science all 'arm-chair scientists'—*i.e.*, all scientists who think while they sit or who sit while they think; that it demonstrated, once and forever, that deductive reasoning is of little use in the discovery of new material truths; and that nothing but observation and inductive reasoning could serve that purpose. It is my purpose to show that this interpretation is logically unsound, and that it has led to many practical disasters, some of which have retarded scientific progress for many years.

What constitutes a genuine experiment? Certainly not random observation. Every genuine experiment is *planned*.

Its plan depends on two sets of assumptions, which the investigator sets up before he can begin to experiment. The plan depends also on his skill in detecting the logical implicates of these assumptions; the plan sets limits to what he can possibly discover by means of the experiment in question.

One set of his assumptions we call *hypotheses*. These are assumptions which the investigator *doubts*. He chooses them for two reasons at once. First, because they agree with all the facts of observation that he knows. Second, because if he uses them as premises, he can deduce from them a true description of some set of those observational facts that interest him. In practice, most hypotheses are compounded of many assumptions set up conjointly: thus, the postulate-system of Hilbert's geometry may be regarded as a single compound hypothesis (8). But whether the hypothesis is compound or simple, the investigator treats it in the same way. He next examines it for its logical implications, knowing that materially true hypotheses cannot imply any material falsehoods. Having discovered some important implicates, he now seeks for a set of facts which, if they exist, will contradict the implicates and thereby falsify the hypothesis. Only at this stage does his study become truly experimental.

It is often said that repetition and control of the conditions of observation are necessary to experimentation. If this were true, then mathematicians, astronomers, and social scientists could not experiment. Actually, the conditions that are jointly sufficient and severally necessary to any experiment are what we have named: *i.e.*, a set of propositions accepted as true; an hypothesis consistent with all these propositions, but from which some of them can be also deduced; detection of other implicates of the hypothesis; an attempt to falsify some implicate.

But besides hypotheses, there is another set of assumptions, which we usually call *presuppositions*. They are assumptions which the investigator *does not doubt*. His reason for not doubting them is often that he does not notice that he has presupposed them. We might well think of presuppositions as being unverbilized components of the investigator's

hypotheses, which function as suppressed premises. But it is most important that the investigator does not doubt them, and that he therefore plans his experiment so as not to test them.

Thus, if the investigator disregards some implications of his hypothesis that he could readily falsify if he recognized them, or if his plan presupposes any falsehoods, his study may fail in the pre-experimental stage, and he need not notice that it has failed. Indeed, he may set up an elaborate set of instruments, make a prolonged series of observations, subject his data to complicated statistical treatment, although it was predestined to yield a false or irrelevant conclusion. Let me give a few examples.

Thirty years ago, I, doubting the validity of certain conclusions of Munks, Rothmann, and Kälischer, concerning the sense of hearing of the dog, undertook to train some animals to distinguish between two nearly pure tones which differed in frequency, and to register their differentiation by turning to the right or the left at the end of a runway, according as the standard stimulus or variable stimulus was presented (3). I failed, although the same dogs learned very quickly to discriminate in this manner between two buzzers, which differed in frequency, and in other respects also. My experiment presupposed that if the animals discriminated frequency-differences without the aid of other characteristics of the sound wave, then they would express their discrimination in the manner which I prescribed. As Dr. Culler's work has demonstrated, the presupposition was false, hence everything that it implied was formally invalid.

Again, beginning 26 years ago (4), I tried to train a dog to distinguish, within limited distances, between two targets having the same area, outline and mean brightness, but having the brightnesses differently distributed, so that the 'positive' target presented a set of black and white horizontal stripes, each subtending as large an angle at the dog's eye as the sun's disc subtends at the earth; the 'negative' target was 'plain.' I required the animal to express his discrimination by choosing the appropriate food-compartment in Yerkes' box. Thus, I

presupposed that if he made this discrimination, he would express it in this manner. He failed, although I caused the targets to be sharply imaged on his retinas; he failed, although he learned to express other visual differentiations than pattern vision by making the prescribed response. Using this presupposition as a suppressed premise, I concluded that it was at least highly probable that the dog is very defective in pattern-vision and that the deficiency is due to a poorly developed retina. But if the presupposition was false, as Lashley, K. U. Smith, and Norman Munn have since demonstrated, in work on other animals, the conclusion which depended on it was formally invalid.

Again, beginning some 15 years ago (6), I caused a group of college men, who were paid for their services, to take a substitution-test about half an hour before they went to bed, and about half an hour after they arose next morning. My purpose was to determine how much they recuperated during $7\frac{1}{2}$ to 8 hours which they spent lying on Mr. Simmons' second-best bed, from the mental impairment which had resulted from 16 to $16\frac{1}{2}$ hours of activity the day before. By this time, I had become a little more skeptical than I used to be of all my plans and procedures. Hence, I was not badly deceived: but it should be noticed that the plan of this experiment presupposed first, that the subject is less fit for performance at the end of the day than at its beginning, and less fit than he will be next morning; and second, that his mental impairment will manifest itself in a decrement of performance in the task which has been prescribed. If I had asserted this compound presupposition, I should have had to conclude that the greater the impairment the better the performance, for of 23 subjects, studied through many months, 22 performed this task better at night than on the morning before, and better than on the morning after; the probability of the difference being due to chance is infinitesimal.

In the same epoch, in a spirit of play (5), I set up a Dunlap association timer and required a few subjects to respond to spoken numbers by naming the result of a predetermined simple operation on them. I worked them sober, and again

after they had ingested the alcoholic equivalent of two highballs. In respect to speed, accuracy and consistency of the subjects' reactions, the differential effect of alcohol was hard to interpret, although both subject and bystanders might agree that the subject when so alcoholized was moderately drunk. But one difference showed itself although the plan did not provide for it. When the subject was sober, he never forgot his instructions; but when he was drunk, he had to remind himself, by speaking to himself, *between stimuli*, what he was to do when the next stimulus was presented. Only by this means, could I, for example, maintain a normal performance. Suppose we had earnestly presupposed that if alcohol produced any impairment, the impairment would express itself in a decrement of speed, accuracy or constancy of response in the prescribed situation, then we should have had to conclude that this much alcohol produced no impairment. The fact that we made note of incidental observations shows that we did not finally make this presupposition.

If time permitted, I could describe to you about eighty studies which I have had to review, in which the impairment resulting from alcohol, prolonged exertion, deprivation of sleep, partial asphyxiation, or of some narcotic was sought for. In each of these studies it was presupposed, falsely, that if the agent of impairment were effective, then its effects would manifest themselves in a decrement of performance of some prescribed function, and that the decrement would be correlated with some characteristic of the agent, such as concentration of the drug in the blood, depletion of the oxygen supply, total work performed expressed in the heat output of the body, or the duration of insomnia. We might include unfavorable illumination in the list, the degree of unfavorableness being sought for in a decrement of the subject's output.

In other words, all these studies presupposed that the subject *would not compensate*, as by releasing new supplies of available energy, by neglecting one set of tasks to perform another, or by substituting one mechanism for another. The fact is that the human organism is most remarkable for its compensations; that when it ceases to compensate, we call it

'dead.' The work of Dunlap, Bagby, and Isaacs (2) on partial asphyxiation illustrates that it is in the mode of compensation, rather than in unfitness to meet an immediate demand, that moderate amounts of impairing agents express their earlier effects. Presuppose that the organism does not compensate, that all the impairment shows itself in a decrement of performance of some selected function, and you will have to draw such conclusions as these:¹

From Miles (10), that a light dose of alcohol produces greater impairment than double the dose.

From certain anti-alcoholic enthusiasts, who qualify as expert witnesses in traffic courts, that the impairment from alcohol is directly proportional to its concentration in some tissue; hence, that given the same concentration at one instant while a man is progressing toward drunkenness, and at another instant after he has passed out but has partially recovered, his fitness at the two instants is equal.

From a certain efficiency expert: that low illuminations impair *visual discrimination* in the gauging of certain small machine parts, while still lower illuminations improve it. Of course, as soon as the illumination becomes too low to permit the operator to judge visually, he begins to rely wholly on the sense of touch.

From Kleitman (9), that because his subjects performed normally in certain prescribed tasks after they had gone 90 hours without sleeping, therefore the loss of sleep did not harm them, even though they were occasionally delirious between tests.

Another example is the work of S. E. Katz and Carney Landis (7) who kept an eccentric subject awake during nearly the whole of 231 hours. Because he maintained performance in all the prescribed tasks, and also excreted no interesting toxins in quantities which they could detect, they concluded that he endured this ten-day vigil 'without any known physiological effect or without permanent change of personality or mental function.' This they concluded, although he also

¹ Some of these authors did not make this false presupposition themselves, and some who implicitly made it refused to draw the necessary conclusion.

became delirious or disoriented between tests, and although during the experiment he built up a well-defined delusion of persecution, directed against one of the experimenters, which lasted several months.

Let me end this list, however, with a parody: namely, an account of an imaginary experiment which I constructed from recent expressions of views held by certain employers and their efficiency experts concerning safety in its relation to hours of labor of interstate truck drivers. Two drivers cover the same 500 mile stretch. Driver *A* stops for rest occasionally, and covers the whole distance without becoming drowsy. Driver *B* makes the same trip, but nods occasionally, through a total distance of one mile. The difference between their mileage rates of nodding is one mile in 500; it is about equal to its own variance, and therefore attributable to chance. Therefore, *B*'s performance within plausible limits may be called equivalent to *A*'s.

I have tried to show, first by analysis and then by example that the pre-experimental assumptions of an investigator determine what he will look for, and accordingly limit what he can possibly find out. To minimize these limitations, and moreover, to prevent himself as well as others, from being deceived by his conclusions, it is necessary that he make full use of the much disparaged procedure of logical analysis and deduction. It may save him much useless labor and may prevent him from doing a great deal of harm.

For example, Galileo was a very keen observer and manipulator. Certain modernists overlook the fact that he was also a *very* competent logical analyst; and that he made ample though not exhaustive use of analytical method. Had he exhaustively analyzed Aristotle's law of falling bodies, he could have shown that it implied two propositions that contradict each other; hence the law itself is meaningless. If his demonstration at the leaning tower was intended for nothing more than to refute Aristotle's law, he need never have made it.²

² I regret that I cannot name the original author of this analysis of Aristotle's law. It came to me indirectly from a high-school teacher through one of his pupils twenty years ago. Given a heavy body *A* and a lighter body *B*, Aristotle's hypothesis implies that *A* will fall faster than *B*. Now join them together, as by a chain; their combined weight is greater than the weight of *A*, hence they would fall faster. But the part *B*,

There are ultra-modernists, of course, who say that they find little or no use for the deductive method; that they are now emancipated from the restrictions of formal logic. They remind one of the engineer who said that in his work he found little or no use for calculus; his reason being, of course, that he did not understand his calculus well enough to distinguish problems in which it is a useful instrument from those to which it is irrelevant. Thus, the editors of some journals of physiology distrust analytical procedure so far that they will not accept a criticism, based on analysis, of the conclusions which one experimenter draws from the results of manipulation, unless the critic submits a set of results of his own manipulation. Such policies as this show a purity of devotion to a principle, or at least to a slogan, corresponding to the ascetic's determination to keep himself 'unspotted from the world'; but like the latter, the policy is anti-scientific in its basis.

Now, having learned that observation and analysis do not antagonize each other, but rather complement each other; that careful observation and careful analysis are each necessary and together sufficient for a successful scientific experiment, what ought we to encourage the young experimenter to learn? He must learn to observe like a Galileo and also to reason like a Galileo.

Our usual curriculum stresses the necessity of accuracy of observation; it does not stress its insufficiency. It stresses the insufficiency of logical analysis, it does not stress its necessity for true experimentation.

I am perhaps a mossback conservative, but if I get a graduate student in psychology who seems to be worthy of the trouble that he will cause me, I try to convince him of the importance of modern logic, *as a necessary instrument for fertile experimentation*. If there is available a competent teacher, I send the student to him; otherwise, I insist that he dig it out for himself, which he can actually do, with such aid as I may incidentally give him. None has yet complained that he found no use for what he learned, although one dis-
being lighter than A , would retard A 's fall, so that the speed of the two bodies joined together should be less than the speed of A . But these two consequences are mutually incompatible.

tinguished philosopher insists that for the sake of both truth and beauty, modern logic ought to be practically useless.

But more: If we wish our children to learn to fiddle, we need a good fiddler to teach them. If we wish our students to become good experimenters, we need to have among our colleagues some competent logicians. Does not this requirement give to the department of psychology, as to the other scientific departments, a legitimate interest in the talents of those who will fill the future vacancies in our own departments of philosophy? We neglect the interests of our own scientific students unless we insist that the personnel include some experts in modern logic and scientific method. And after we get these experts, should we not insist that they practice their profession some of the time and not preoccupy themselves wholly with the Absolutes of Hegel, or the paradoxes of Nicolas Cusanus, or with the various possible meanings of an epigram which some pompous, pediculous, sudoriferous, Grecian pederast uttered 2500 to 3000 years ago but did not trouble himself to explain. Such questions as these might well claim all the time of some philosophers and some of the time of any philosopher, but they should not claim all the time of all the philosophers on any campus. In a word, since the scientists have discovered some useful work for philosophers to do, ought not the philosophers spend part of their time in doing it?

BIBLIOGRAPHY

1. ARISTOTLE. *Physics*. Book 4, ch. 8.
2. BAGBY, E. The psychological effects of oxygen deprivation. *J. comp. Psychol.*, 1921, 1, 97-113.
3. JOHNSON, H. M. Audition and habit-formation in the dog. *Behav. Monogr.* No. 8, 1913.
4. —. Visual pattern discrimination in the vertebrates. 5. A demonstration of the dog's deficiency in detail vision. *J. Anim. Behav.*, 1916, 6, 205-221.
5. —, SWAN, T. H., & WEIGAND, G. E. Sleep. *Psychol. Bull.*, 1926, 23, 482-503.
6. —. The real meaning of fatigue. *Harper's Mag.*, 1929, 158, 186-193.
7. KATZ, S. E., & LANDIS, C. Psychologic and physiologic phenomena during a prolonged vigil. *Arch. Neurol. Psychiat.*, 1935, 34, 307-316.
8. KEYSER, C. J. *Pastures of wonder*. New York: Columbia Univ. Press, 1929.
9. KLEITMAN, N. The effects of prolonged sleeplessness on man. 1. *Amer. J. Physiol.*, 1923, 66, 67-92.
10. MILES, W. R. Alcohol and human efficiency. Publ. 333. Carnegie Institute of Washington, 1924.

ATOMISM VERSUS GESTALTISM IN PERCEPTION

BY E. B. SKAGGS

Wayne University

Following the advent of Gestalt Psychology the writer has watched with great interest the clashes between members of this point of view and those who represented an earlier, more analytical point of view as regards the receptive processes. These last-named individuals of the 'older school' are scarcely to be designated as 'atomists,' although they were inclined to push analysis of mental states and processes back to rather simple constituent elements. In fact we doubt seriously if there ever was a thorough-going atomist among psychologists since the time of James and Wundt. On the other hand, there is no doubt that the Gestalt view has contrasted rather sharply with the view of sensory processes and perception as held by most American psychologists up to the time of the Gestalt school.

The writer has long since come to the conclusion that both Gestaltists and atomists (if there ever were 'extreme' atomists) represent extremes of mental organization and synthesis. In a recent paper¹ Kuo makes the following statement: "In other words, total pattern and local reflexes are merely theoretical abstractions and represent the two types of behavior; in between there are other movements of varying degrees of complexity, none of which can be strictly grouped under the category of total pattern or that of local reflex."² Kuo, the strict behaviorist, is speaking here of motor activity and is concerned with the question whether in the development of the human organism, mass-action or segmental acts come first. However, if one should substitute the term 'Gestalt' for 'total

¹ As the writer finished the first rough draft of this paper, this article by Z. Y. Kuo came to his attention: Total pattern or local reflexes; *PSYCHOL. REV.*, 1939, 46, 93-122. Kuo, speaking strictly in terms of behavior, has stated a position for objective behavior which the writer holds for the receptive psycho-neural processes.

² Z. Y. Kuo. *Op. cit.*, p. 118.

behavior pattern' and 'atomism' for 'local reflexes,' this statement would express precisely the view of the writer in the realm of the receptive processes.

DEVELOPMENTAL SPECULATION

At the outset we make the assumption that all conscious experiences or phenomena of awareness are merely an aspect of the neurological processes of the nervous system in general and the cerebrum in particular. Also we assume that, of necessity, all conscious processes must depend upon and parallel in development the growth and development of the nervous system. The emergence of the phenomena of consciousness or personal experience in their various complexities and organizations must then depend upon the complexities and organizations of the nervous system. For the psychologist, who is interested in conscious phenomena, the developmental picture of consciousness is of extreme interest and importance.

Unfortunately, long before the psychologist can reach and investigate the growing infant, it has gone far in its development as a psycho-neural organism and the important pre-developmental picture of consciousness is lost. What kind of a consciousness the growing embryo or infant has from time to time in the pre-natal development must forever, we believe, remain in the realm of speculation or await, for indirect interpretation, the development of our knowledge of neural anatomy and physiology. Unfortunately, again, developmental anatomy and physiology fail to give us much enlightenment as to the mental aspects of the infant's early development.

Anatomists seem to agree that the projection areas of the cerebrum develop first; at least myelinization seems first to occur in these areas. Myelinization presumably would mean canalization or grooving of nerve processes, thus making for specificity of process. One might argue that, in early developmental stages, any afferent excitation of any projection area in the cerebrum would at once spread in a diffused way over the entire cerebrum, producing a total and rather undiffer-

entiated 'mass activity' or 'mass consciousness.' On the other hand one might go to the other extreme and hold that, in the early stages of cerebral development, cortical action is limited to specific bits of projection area, producing point-like localized sensory effects. In either case there would be a decidedly small degree of mental organization or patterning, if any, present. If any honors are to be given at all at this point, we would say that they should be given to the atomistically inclined speculator. Relatively undifferentiated, diffused, mass-activity on the part of the cerebrum would be something of an antithesis of the usual Gestalt view, as I understand it, of organization or configuration.

However, at these very early stages of psycho-neural development, we are inclined to believe that some middle ground between the two above stated views is more typical of the picture of mental life. Granted differential values of intensity to the various stimuli which act upon the infant's receptors, then one might reasonably think of one area of *greatest activity* in the cerebrum at any given moment. In the older way of thinking, this would mark the emergence of the phenomenon of attention, whereby one local psycho-neural event dominated in activity at any given moment. On the other hand 'spread' of neural energy would probably take place over the cerebrum and so the localization would be only relative, chiefly in terms of intensity perhaps. Since several projection areas in the infant would be stimulated at the same time, especially the interoceptors and the proprioceptors, there would be a general brain activity and a rather wide-spread sensory consciousness.

In the light of the above speculation, one might conceive of a sensory consciousness, in the very earliest stages of phenomenological-neurological development in the human embryo, as having all degrees of organization and specificity. Consciousness might well be pictured at one moment as an undifferentiated, disorganized, mass-consciousness and at another moment as a highly focalized and limited consciousness characterized by one mental event, as an intense sound, standing out above the background.

Various degrees of Gestalt or configuration would have to wait, presumably, upon the establishment of functional connections in the cerebrum, a process partly dependent upon mere maturation and partly dependent upon learning. In the older terminology these functional connections were called 'associations,' a term which we still believe to be essential in psychology. One may well speculate whether, as the physiological mechanisms develop, systems of spatially related nerve-elements become coordinated, integrated, or patterned to various degrees. With relatively complete neural development one has at least the possibilities for large patterns or Gestalten.

By the time that the Gestalt psychologist gets his experimental fingers upon the infant or other animal the process of growth, through both maturation and learning, is far along. It seems to the writer very questionable for anyone to assert that the first conscious experiences of the growing infant are Gestalten, unless he be willing to allow far more elasticity in the meaning of the term than is usual on the part of the Gestaltist. Perhaps, with the increase of knowledge of developmental anatomy and physiology, we may be able to reconstruct the conscious life of the developing infant. At present, as Kuo has pointed out in the case of action, developmental neurology has little to offer us.

PHENOMENOLOGICAL ANALYSIS

Let us now turn to the receptive processes as we meet them in the developed human consciousness. Introspection and verbal report furnish our data for discussion. Again we shall conclude that both Gestaltism and atomism represent extremes of mental organization, that there are all degrees of 'size of operational mental units' between the extremes, and that there are all degrees of Gestalten themselves.

It seems pertinent to bring up the problem of what constitutes Gestalt, configuration, or pattern, since there is much danger of confusion lurking in these terms unless they are carefully defined. In order to discuss the problem, a series of

experiments reported by Helson and Fehrer in 1932³ will be considered.

These experiments were designed to test certain assertions which were made by the Gestalt school regarding the nature of perception. The Gestalt school had contended that every sensory experience was a structure, a configuration, or a pattern. The experimenters had their subjects observe lighted windows of various geometrical patterns or forms. The light was first presented in sub-liminal intensity and they gradually stepped up in intensity until it was well above the threshold for light discrimination. The window was made brighter and brighter until every subject could definitely tell just what geometrical pattern was used. The subjects merely observed and gave their reports. As the light-intensity of the stimulus was increased from sub-threshold to threshold and super-threshold values, the following typical reports were made. First, a light developed on a field of darkness but it was a 'light without form or pattern.' Second, with further increase in intensity of light, the subjects reported a 'vague and indistinct form or spread-out-ness.' Third, with further increase in intensity of the light, all subjects perceived a definite form or pattern, as a square, a triangle, or a circle.

Since a bare sensory quality of brightness was first experienced without pattern or form, Helson and Fehrer argued that their experiment cast considerable doubt upon the accuracy of the Gestalt claims that all sense-impressions come as definite Gestalten or patterns. However, did the experiment really challenge the Gestalt view? It most certainly did if by form or pattern the Gestalt group meant form or pattern as used by most persons. If, on the other hand, the Gestalt group used the term form or pattern to signify merely the fact that one perceives a diffuse light blending into the surrounding dark background, then the experiment in no sense touched the Gestalt claims.

Here is the crux of the whole matter. Just what does Gestalt, object-on-a-ground, configuration, form, or pattern mean? If, in this particular case, the Gestaltist uses the

³ H. Helson & E. V. Fehrer, *Amer. J. Psychol.*, 1932, 44, 79-102.

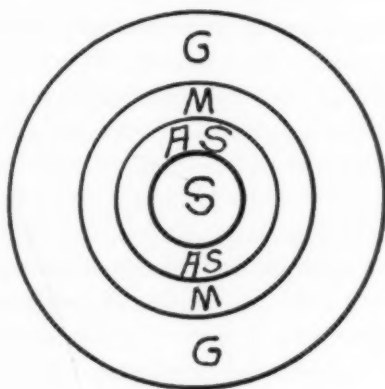
word pattern merely to state the fact that the observer could distinguish between two sets of experiences, brightness and darkness, he differs very little indeed from the older, more atomistically inclined psychologist who spoke of a sensory experience standing out in attention. For most persons, however, form or pattern means a definite boundary line between object and ground. So the subjects in the experiment just cited said that the light was without pattern or form until such a definite boundary line developed. These results suggest that there are all degrees of organization and synthesis of sensory experiences, ranging from relatively unorganized and small focalized experiences to highly organized, complicated Gestalten.

Experiments of the Gestalt school have shown that even very young children possess more organization in their receptive processes than the older, more analytically-inclined psychologists were prone to admit. They have clearly indicated the capacity of the young child in many situations to grasp large organizations more easily than small 'objects.' It also seems well established that the child perceives ready-made Gestalten in terms of the given sense-modality, as when, for example, it perceives a well-lighted, triangular form. Whether all sensory experiences are so ordered and patterned is another matter.

As the writer views perception there are several aspects involved. This may be illustrated by reference to the following diagram. First there is that which is given immediately and directly through the sense-modality involved. This is represented by an *S* in the diagram, the *S* standing for sensory datum. Within *S* there may be varying degrees of organization, ranging from a rather isolated sensory experience, as a sharp pain dominating attention, to a series of pains from several areas which are integrated into one big massive pain. *S* may represent the simple tone of a tuning fork or the combination of several tones. Again, *S* may represent a tiny point of light in the darkness or a whole window of light.

In any perception, however, there are often accruments from other sense-modalities, parts of the perception which

are not given directly through the arousal of the given projection area or sense-modality. Here past experience, learning, or association seems to play a part and evidently a synthetic operation must have occurred in the past (the aversion of the Gestaltist to 'creative synthesis' notwithstanding). If one observes a cake of ice, one is aware of coldness and hardness. These are essential properties of the total perception, but quite obviously they are accruments from other sense-modalities which, at the time, do not involve corresponding receptor activity. We cannot see that the Gestalt view adequately accounts for these accruments, which we have shown in circle *AS* in the diagram (*AS* for associated



sensory experience). We believe the Gestalt view would be much stronger if the Gestalt psychologists would frankly admit the building or integrating processes (association?) which are here implied.

In our diagram *M* stands for a third aspect of perception, namely, its meaning. If one admits that meaning is an essential part of any perception, then we may think of a linear series of degrees of complexity and organization. Also, one must face here the definite problem of learning and past experience. If, on the other hand, the perception is thought to consist of only *S* or of *S* and *AS* together, then meaning becomes superfluous. While in abstract analytical thinking we may strip a perception of its meaning, actually we find

these meanings to be an essential part of the total perceptive experience. A printed form, as a word on a page, is a bare visual perceptual Gestalt. Its significance or meaning seems as essential a part of the actual use of the symbol as the symbol itself.

It is in terms of *AS* and *M* that our perceptions undergo most change, we believe. These changes occur as a result of experience and learning. Both additions and subtractions may thus occur.

Finally, we note in our diagram *G* which stands for the 'rest of conscious awareness' at the moment. For the Gestaltist a part, at least of this content is the 'ground.' In our own mode of thinking, *S*, *AS*, and *M* stand out most clearly in consciousness, a phenomenon which has usually been called attention. The attentive act is some psycho-neural process (perhaps involving inhibition and facilitation?) whereby the particular perception stands out above *G*.

The Gestaltist seems to have only *S* and *AS* as content of the 'figure' or 'object.' His 'ground' includes some or all of *G*. *M* is left sadly unaccounted for, we believe. The only difference between atomism and Gestaltism would seem to lie in the size of *S* and *AS*. Actually, we believe that these are essentially differences in degree of organization, one representing a simple operational unit, one a more complex unit.

SUMMARY

The writer has contended that we should look upon atomistic and Gestalt views as representing extremes in the perception process. The implication has been that Gestalt is now being assimilated into the main stream of psychology and will lose its identity, just as did behaviorism and other 'isms' of the past. Its contribution has been chiefly that of forcing us to realize that larger organizations may be experienced than the older extreme views considered possible. Human consciousness is very complex and the condition of perceptual organization may vary from almost chaos and atomism to large and stable integrations or patterns.

[MS. received February 12, 1940]

PSYCHOPHYSICS AND MENTAL TEST THEORY: FUNDAMENTAL POSTULATES AND ELEMENTARY THEOREMS

BY CHARLES I. MOSIER

University of Florida

The two fields of psychophysics and mental testing have pursued somewhat independent courses of development, utilizing, it is true, certain common statistical concepts, but touching one another only occasionally in their respective courses (2). Each field has developed its own theorems, its own techniques for solving its problems, with little transfer from one field to another, except where the individual worker has been led by analogy to carry over the technique of one field to that of the other. It is the purpose of this paper to reduce both disciplines to their common postulational basis, in the terms of their elements, definitions and assumptions, to deduce certain of the better known theorems of each field from these definitions and assumptions, and to show, since the two fields have a common basis of data and postulates, that it is possible by transposing certain terms, to restate the theorems of psychophysics for the mental test situation, and the theorems of mental test theory for the situations of psychophysics. By making available for mental testing the techniques and theorems developed by workers in psychophysics, new possibilities are opened up, and similarly, the utilization of mental testing methods and theorems may well extend the range of psychophysics in a systematic manner. Certain of the theorems of one field as restated in terms of the other will, of course, be trivial, and certain others of theoretical interest with no practicable applications; others, it is hoped, will be of considerable value.

Psychophysics and mental test theory have as their common data the responses of individuals to stimulus situations. This response is not a function of the individual alone, nor of

the situation alone, but represents a *relation* between individual and situation. In psychometric methods we seek a measure of some particular attribute of the stimulus along a particular psychological continuum; in testing we seek the position of the individual along a trait continuum.

When we ask a hundred individuals to respond to a hundred items it may be under circumstances such that we pool the responses of the hundred judges in order to derive measures of the hundred stimuli; or under circumstances such that we pool the responses of the hundred situations to obtain test scores for each of the hundred persons. In the first case—psychometrics—one hundred subjects have judged (or tested) a set of stimuli (items). In the second case—mental testing—one hundred items (stimuli) have tested (judged) a group of persons. The distinctions between the two situations are first whether we consider a row or a column of the table of responses of persons to items, and second whether we are justified in making assumption * (6) or * (6a) (*Vide infra*, p. 359).

The parallel may be made clearer by presenting some of the familiar terms of the two fields in parallel columns.

Psychometrics	Mental Test Theory
stimulus.....	person, individual
judge.....	item
scale value.....	trait score
ambiguity	}..... variability
precision	
discriminal dispersion	
to judge or rate.....	to test
attribute.....	trait

Thus *items test a person's score* with a *variability* dependent on the *individual* and the *trait* tested, for a given set of items. *Judges rate the scale values* of a *stimulus* with a *precision* dependent upon the *stimulus* and the *attribute* for a particular set of judges.

As in psychometrics we consider the ambiguity of the stimulus—its discriminial dispersion—so in test theory we must, or should, take into account the variability of the individual. Furthermore, just as a stimulus is variable with respect to a particular group of judges, so is an individual

variable with respect to a particular set of items, at least until his constancy has been empirically demonstrated for each case. Conversely, as the reliability of a set of judges as sources of data for psychometrics may vary from one stimulus to the next, so does the reliability of a set of items (a test) vary from one individual to the next. What we conventionally term 'the reliability of a test' is thus once more recognized to be its average or composite reliability for a particular set of individuals. When, however, we speak of the reliability of a psychometric scale value or of an individual's score, that is a value which varies from one individual or one scale value to the next. It is only imperfectly represented by the composite reliability, even for the population of persons or the set of stimuli, and it depends, not on the score or the scale value, but on the ambiguity of the stimulus or the variability of the individual.

For convenience we shall utilize the symbol R_{ij} to represent the response of the i -th individual to the j -th situation. If we consider the responses of a number of individuals to a series of stimuli, we may arrange those responses in a matrix. Let a stimulus situation, designated by the subscript j or k , be presented to an individual, h or i , for the response, R_{ij} . This response is, as has been noted above, a function of both the individual and the situation. Let there be a total of n situations, each presented to N individuals. As the stimuli, j , vary from 1 to n and the individuals, i , vary from 1 to N , we obtain nN responses, R_{ij} , which may be arranged in the matrix form:

$$(1) \quad R = \begin{bmatrix} R_{11} & \cdots & R_{1j} & R_{1k} & \cdots & R_{1n} \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ R_{h1} & \cdots & R_{hj} & R_{hk} & \cdots & R_{hn} \\ R_{i1} & \cdots & R_{ij} & R_{ik} & \cdots & R_{in} \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ R_{N1} & \cdots & R_{Nj} & R_{Nk} & \cdots & R_{Nn} \end{bmatrix}$$

If we consider a single row of the matrix R we have a series of situations ($j = 1, 2, \dots, n$) presented to a single individual, i . A single column represents a single stimulus, j , presented to a series of individuals ($i = 1, 2, \dots, N$). This matrix is mathematically transposable with respect to persons and

situations, and from this transposability we may draw a number of interesting parallels between the two fields of psychophysics and mental test theory. It is possible, by making certain assumptions in transposed pairs, substituting row for column—individual for stimulus—to deduce parallel theorems for these two fields of quantitative psychology. This paper proposes to set forth explicitly the terms, definitions and assumptions which underly both fields, and those which are transpose-pairs for the two fields, and to deduce certain of the important theorems of the two fields from these elements, definitions and assumptions.

With reference to the matrix R we may suppose that each response R_{ij} is the composite (as opposed, for the time being, to the additive sum) of two components, one a fixed component which remains stable for a given individual and a given stimulus over a finite time interval, which component we shall term x_{ij} , and a second, variable component which corresponds to what is commonly understood as chance error, and which we shall term e_{ij} .

To summarize the notation thus far introduced we may consider the elements under consideration as individuals, situations, responses.

R represents a response.

h, i are subscripts denoting individuals, of whom there are N .

j, k are subscripts denoting situations, of which there are n .

R_{ij} is the response of the i -th person to the j -th stimulus.

x_{ij} is the constant component or 'true value' of R_{ij} (cf. [4]).

e_{ij} is the variable component or 'chance error' of R_{ij} (cf. [4]).

We here introduce the first assumption necessary to the derivation of the theorems of both psychophysics and mental test theory. (Throughout, statements which are conceived to be assumptions or postulates will be denoted by an asterisk; the \equiv symbol will be used to indicate a definition.)

*(2) R_{ij} , x_{ij} , and e_{ij} are expressible in additive numbers.

This assumption is requisite to every further proposition. With this assumption we may express the definitions of x_{ij} and e_{ij} more exactly. If R'_{ij} , x'_{ij} , and e'_{ij} represent the values for the response of the i -th individual to the j -th situation on a second presentation after a finite time interval, then:

$$(3) \quad x_{ij} \equiv x'_{ij}, e_{ij} \neq e'_{ij}.$$

From the assumption of equation *(2) and the definitions of R , x , and e , we obtain the following proposition which is fundamental to both psychophysics and mental test theory:

$$*(4) \quad R_{ij} = (x_{ij}) + (e_{ij}).$$

A further assumption necessary to both fields is that x_{ij} and e_{ij} can be represented on the same linear continuum, that is:

$$*(5) \quad e_{ij} \equiv \Delta x_{ij}.$$

The assumptions in propositions (2) and (5) and the definitions thus far introduced constitute the common basis of psychophysics and test theory. From this point on definitions, assumptions and deductions are made separately, and first for the theorems of psychophysics. Later, when the corresponding propositions are made for test theory, it will be seen that each proposition is the transpose of that for psychophysics, in that person and stimulus, i and j , will be interchanged.

To show the application of these concepts, certain parallel theorems are deduced, first for psychophysics, then for test theory.

The basic assumption of psychophysics and scaling theory is that there exists a scale-value which is representative of the subject-stimulus relation, which depends only on the stimulus situation. In other words, we assume the existence of a scale-value. In our notation this assumption becomes:

$$*(6) \quad x_{ij} = x_{hj} \equiv x_j.$$

This assumption is tacitly made in all psychophysical investigations. The individuals are so chosen, with respect to the particular stimuli being scaled, that for any given stimulus

the true response of one individual is the true response of any other; the actual responses of two individuals to the same situation are assumed to differ only by chance error. As an instance of this, in scaling colors for æsthetic value, the responses of a color-blind subject would be rejected. Furthermore, in the same problem, the subjects would be so selected as to be homogeneous with respect to artistic training if that is thought to affect the constancy of x_j .

Proposition *(6) is equivalent to saying that in the matrix form \bar{X}_{ij} , all values within a column are equal.

We shall introduce here a definition,

$$(7) \quad \frac{1}{N} \sum_i^N e_{ij} \equiv \bar{e}_j$$

and one further assumption,

$$*(8) \quad \lim_{N \rightarrow \infty} \frac{1}{N} \sum_i^N e_{ij} \equiv \lim_{N \rightarrow \infty} \bar{e}_j = 0.$$

Proposition *(8) is the familiar assumption that the sum of a large number of chance errors becomes zero.

We come now to the first theorem of psychophysics.

I. *The true response value of a stimulus is given by the mean of the responses obtained from a large number of individuals.*

If we sum proposition (4) over the N individuals, we obtain

$$(9) \quad \sum_i^N R_{ij} = \sum_i^N x_{ij} + \sum_i^N e_{ij}.$$

Dividing (9) by N and recalling *(6) and (7), we have

$$(10) \quad \frac{1}{N} \sum_i^N R_{ij} = x_j + \bar{e}_j.$$

Now, under the assumption *(8), as N becomes large

$$(11) \quad \lim_{N \rightarrow \infty} \frac{1}{N} \sum_i^N R_{ij} = x_j.$$

This is essentially the basis of the methods of psychophysics.

We here define a new term as the standard deviation of a

column of the error matrix, E_{ij} ,

$$(12) \quad \epsilon_j^2 \equiv \frac{1}{N} \sum_i^N e_{ij}^2 - \bar{e}_j^2.$$

Then, substituting (4) and (7) in (12), we have:

$$(13) \quad \epsilon_j^2 = \frac{1}{N} \sum_i^N (R_{ij} - x_{ij})^2 - \frac{1}{N^2} \left(\sum_i^N R_{ij} - \sum_i^N x_{ij} \right)^2.$$

Expanding (13), we obtain:

$$(14) \quad \epsilon_j^2 = \frac{1}{N} \sum_i^N R_{ij}^2 - \frac{2}{N} \sum_i^N x_{ij} R_{ij} + \frac{1}{N} \sum_i^N x_{ij}^2 \\ - \frac{\left(\sum_i^N R_{ij} \right)^2}{N^2} + \frac{2}{N^2} \sum_i^N x_{ij} \sum_i^N R_{ij} - \frac{\left(\sum_i^N x_{ij} \right)^2}{N^2}.$$

If now we substitute from assumption *(6) in (14) we have:

$$(15) \quad \epsilon_j^2 = \frac{1}{N} \sum_i^N R_{ij}^2 - \frac{2x_j}{N} \sum_i^N R_{ij} + x_j^2 - \left(\frac{\sum_i^N R_{ij}}{N} \right)^2 \\ + \frac{2Nx_j}{N^2} \sum_i^N R_{ij} - \frac{N^2 x_j^2}{N^2}$$

or, on collecting terms,

$$(16) \quad \epsilon_j^2 = \frac{1}{N} \sum_i^N R_{ij}^2 - \left(\frac{\sum_i^N R_{ij}}{N} \right)^2.$$

Proposition (16), derived by a use of assumption *(2), of course, and *(6), states that the standard deviation of the errors made by the N individuals in judging the j -th stimulus situation is the standard deviation of the j -th column of the response matrix, R_{ij} . The term ϵ_j^2 is $h/\sqrt{2}$, where h is here the measure of precision found in the older psychophysics.

Now if we consider two responses R_{ij} and R_{ik} , made by the same individual, then,

$$(17) \quad R_{ij} < R_{ik} \equiv x_{ij} + e_{ij} < x_{ik} + e_{ik}. \quad \text{by (4)}$$

If we subtract x_{ik} from both sides of the right-hand inequality,

then

$$(18) \quad R_{ij} < R_{ik} \equiv x_{ij} - x_{ik} + e_{ij} < e_{ik}.$$

Similarly subtracting e_{ij} from both sides of the right-hand inequality,

$$(19) \quad R_{ij} < R_{ik} \equiv x_{ij} - x_{ik} < e_{ik} - e_{ij}.$$

Let us now assume that the frequency function of e_{ij} for any stimulus j is Gaussian (other frequency functions might similarly be assumed).

$$*(20) \quad \phi(e_j) = \frac{1}{\sqrt{2\pi\epsilon_j^2}} e^{-(e_j^2/2\epsilon_j^2)},$$

where $\phi(e_{ij})$ is the frequency distribution of e_{ij} . This proposition depends on *(2), *(8) and (12).

Now the proportion of individuals judging stimulus j less than stimulus k , designated by: $P_{(R_{ij} < R_{ik})}$ or $P_{j < k}$ is given by:

$$(21) \quad P_{j < k} = P_{(e_{ij} - e_{ik} > x_{ij} - x_{ik})} = \int_a^\infty \phi(e_j - e_k),$$

where the lower limit of integration, a , is given by equation (22), below. If $P_{j < k}$ is given from data, we may obtain from the tables of the normal probability integral $a_{jk} \equiv \phi^{-1}(P_{j < k})$ and then

$$(22) \quad \frac{x_{ij} - x_{ik}}{(\epsilon_j^2 + \epsilon_k^2 - 2r_{jk}\epsilon_j\epsilon_k)^{1/2}} = a_{jk},$$

where r_{jk} is the correlation between the errors of response of the N individuals for the two stimuli, j and k . Recalling *(6), we have

$$(23) \quad x_j - x_k = a_{jk} \sqrt{\epsilon_j^2 + \epsilon_k^2 - 2r_{jk}\epsilon_j\epsilon_k},$$

which is Thurstone's Law of Comparative Judgment (3).

Turning now to mental test theory we may, by making the transposed assumptions and definitions develop similar theorems.

If, now, we turn to the mental test situation, we may, by transposing i and j , persons and stimuli, develop corresponding theorems. We make first the assumption that there exists,

in a series of n responses of an individual a value which is the same for all the stimuli.

$$*(6a) \quad x_{ij} = x_{ik} \equiv x_i.$$

This assumption underlies all test theory. Our test situations are so selected that a *single* ability is thought to underly them. We do not include within a series arithmetic problems and personality questionnaire items, and our inclusion of arithmetic problems and vocabulary questions depends on the assumption, tacit or expressed, that there is, for one individual, a single number which expresses his score in the single ability measured by both sorts of items.

Similarly we introduce the definitions,

$$(7a) \quad \frac{1}{n} \sum_j e_{ij} \equiv \bar{e}_i,$$

make the transpose assumption,

$$*(8a) \quad \lim_{n \rightarrow \infty} e_i = 0,$$

and by the same reasoning as in (9) and (10) we have the theorem

$$(11a) \quad \lim_{n \rightarrow \infty} \frac{1}{n} \sum_j R_{ij} = x_i.$$

This is precisely the concept of true score designated by Kelly as x_∞ . We usually do not concern ourselves with the score x_i , but make comparisons between individuals for a constant number of stimuli on the basis of nx_i .

We may also define the concept of the standard deviation of the errors of response for a particular individual in a single test of n items by

$$(12a) \quad \epsilon_i^2 = \frac{1}{n} \sum_j e_{ij}^2 - \bar{e}_i^2.$$

While this concept has not to my knowledge been defined previously, it appears to be a concept which might prove extremely useful. There is no more reason to suppose that the error of a test score should be the same for all individuals for a given test than that the error in scaling all stimuli for a given group of judges should be the same.

Again by steps parallel to propositions (13), (14) and (15) we deduce

$$(16a) \quad \epsilon_i^2 = \frac{1}{n} \sum_j R_{ij}^2 - \left(\frac{\sum_j R_{ij}}{n} \right)^2.$$

Such a definition of the 'standard error of a test score' is not equivalent to the customary expression:

$$\text{S.E.} = \sigma \sqrt{1 - r_{tt}}.$$

Furthermore, the expression in (16a) is adapted to tests where the score varies over several steps, rather than to those where items are scored 1 or 0. An expression for the pass-fail items has been developed, and will be introduced later. For a test whose items are scored right or wrong

$$\epsilon_i = \sqrt{\frac{R}{n} \frac{W}{n}} = \sqrt{p \cdot q} = \sqrt{1 - \frac{R}{n}}$$

and is seen to depend only on number right. Our method of scoring has blinded us to the existence of the concept. The value ϵ_i indicates the variability of the individual's test score as successive samples of test items are drawn from the same universe of test items. The more familiar concept, $\text{S.E.} = \sigma \sqrt{1 - r_{tt}}$, indicates the variability of any individual's test score as the *test* is replaced by a second similar test with the same mean, S.D., and reliability coefficient.

The new concept, ϵ_i^2 , relates the notion of a mental test to the idea of a test found in other fields. The reliance which can be placed on a test is thus made to depend, at least in part, upon the homogeneity of the field tested, and the representativeness of the samples drawn from that field. This can be related still further to the one-dimensionality of the items. Thus if our test is wholly a vocabulary test, we would expect a smaller ϵ_i^2 than if the test consisted of vocabulary, arithmetic, and analogies. There are two factors which enter into the size of ϵ_i^2 for a particular test: *a*, the number of 'primary traits' included in the field from which the items are sampled; and *b*, the magnitude of the chance response errors to par-

ticular items. In the case of testing, our field, or universe, is all behavior items expressive of a common trait, and our sample of behavior items drawn from that universe is the test in question. The application of the concept to, *e.g.*, achievement examinations or comprehensive examinations is rather obvious.

If we made the assumption that, in a given series of test-items, an individual's chance errors are normally distributed,

$$*(20a) \quad \phi(e_i) = \frac{1}{\sqrt{2\pi\epsilon_i^2}} e^{-(e_i^2/2\epsilon_i^2)},$$

we may, by means of the transposes of propositions (17) to (22), arrive at the theorem:

$$(23a) \quad x_h - x_i = a_{hi} \sqrt{\epsilon_i^2 + \epsilon_h^2 - 2r_{ih}\epsilon_i\epsilon_h},$$

where r_{ih} is the correlation between the errors, for a single series of n items, of the two individuals, and a_{ih} is determined by the proportion of items in which the score of individual h was greater than the score of individual i . This theorem seems not to have been explicitly stated previously.

It thus appears that a person has, for a single test, two characteristic values, x_i and ϵ_i , rather than the single test score conventionally given. It may be that for a single test, ϵ_h equals ϵ_i , but this is a special case. The use of all-or-nothing scoring for test items leads, of course, to a suppression of ϵ_i , since it becomes, for that case, a function of the proportion of items scored as 'correct.' It is probably this fact which accounts for its having been hitherto neglected. When, however, deductions are made for wrong answers, it is this concept which would permit the differentiation between the individual who gets p items correct by attempting only p items, and the individual who similarly gets a score corresponding to p items correct, but by attempting three times as many items.

Equations (11) and (16), derived from assumptions *(2) and *(6) contain the essence of the psychophysical method of equal-appearing intervals. These propositions were shown to have their transposes for test theory in equations (11a) and (16a).

In summarizing the present paper, we may point out that the paper has established the transposability of theorems in psychophysics and mental test theory, has reduced both fields to a common postulational basis, has derived the basic theorems of the method of equal-appearing intervals and the law of comparative judgment from the basic postulates and specific assumptions, and has derived the parallel theorems for the mental test field. It has added a new concept to the field of mental test theory—the variability of an individual score—and has pointed out the possibility of scaling individuals by the analog of the law of comparative judgment.¹

REFERENCES

1. BURT, C., & STEPHENSON, W. Alternative views of correlations between persons. *Psychometrika*, 1939, 4, 269-281.
2. GUILFORD, J. P. *Psychometric methods*. McGraw-Hill, N. Y., 1936, pp. 566.
3. THURSTONE, L. L. A law of comparative judgment. *Psychol. Rev.*, 1927, 34, 273-286.
4. THURSTONE, L. L. *Vectors of mind*. Univ. of Chicago Press, Chicago, 1935.

¹ The author expects in the near future to derive the method of successive intervals for the two fields, to relate the concept ϵ_i^2 to the conventional concept of reliability of a test score, to present methods for handling the concept of individual variability with the usual, all-or-none method of scoring test items, and to relate the matrix of responses, R , to factor theory (4), and to the inverted factor theory presented by Stephenson and Burt (1).

[MS. received February 19, 1940]

